VOL. 31, No. 6 November, 1924

THE PSYCHOLOGICAL REVIEW

# A PSYCHOLOGY WITHOUT HEREDITY

BY ZING YANG KUO

*Fuh Tan University, Shanghai, China*

## A CONFESSION OF FAITH

In view of the fact that at present there is no generally accepted point of view in psychology, it seems necessary that one should state definitely where he stands in the science before he can proceed to the discussion of a specific psychological problem; for unless there is an agreement on a general viewpoint among those who join in the discussion, there is little hope of settling the question at issue. I feel obliged, therefore, to make a confession of my psychological faith before I take up the problem of inheritance of behavior, which is the main theme of the present paper.

I shall define psychology as *the science which deals with the physiology of bodily mechanisms involved in the organismic adjustment to environment with special emphasis on the functional aspect of the adjustment.* (By functional aspect, I mean the effect, or result, or adjustment-value—positive, negative or indifferent—of a response which establishes a new functional relation of the reacting organism to its environment, social or otherwise.) Psychology adopts the methods of the exact sciences, stressing the supreme importance of objective and quantitative experiments for permanent progress of the science. Its subject-matter—behavior—is solely physical and mechanical events. It denies (*but does not disregard*) the existence of anything mental or subjective; the so-called consciousness, if it exists at all, must be reducible

to physical terms and capable of objective and quantitative treatment when we have better methods and technique than the ones in existence at present; there is nothing unique about consciousness, nor does it need any special explanation (**16**).

It naturally follows that in any kind of psychological discussion, the laboratory viewpoint should always be kept clearly in mind. Any controversy in psychology must be capable of promoting experimental researches so that the issue can be settled in the laboratory, or it must at least have some particular value for laboratory procedure. Otherwise there is no justification for the existence of any such controversies or problems in the science. It was for this reason that I attempted to repudiate the concept of instinct (**6**, **7**) and it is this same reason which has led me to question the validity and usefulness of the whole concept of heredity in a laboratory psychology.

From the above brief statement—which is, by the way, obviously dogmatic and mechanistic in the extreme and which will certainly shock our metaphysical opponents, notably McDougall—as a confession of my general viewpoint in psychology, one can readily see that my controversy in this paper is with the strict behaviorists alone, for they are the only ones who are likely to agree with me on such a platform. *It is the main thesis of the present paper that in a strictly behavioristic psychology, with its emphasis on laboratory procedure and with its insistence on physiological explanation of behavior, there is practically no room for the concept of heredity.* With McDougall and other vitalists and with the mentalists I have no quarrel. My chief difficulty with this group of writers is purely a metaphysical one; and so long as the philosophy of my psychology is irreconcilable with theirs, I do not think we can get together and discuss profitably the problem of heredity or any other specific problem in psychology.

To repeat my main thesis, I insist that the problem of psychological heredity should be attacked solely from the laboratory standpoint; the concept of heredity in psychology must be a proved or provable fact in the psychological lab-

oratory, or it must at least be a valuable assumption for laboratory procedure; beyond the laboratory viewpoint I confess my inability to discuss the problem.

## The Difficulty of the Concept of Heredity in Psychology

For most of the strict laboratory students of heredity, the problem of psychological inheritance seldom exists. Their primary interest lies in the inheritance of morphological features of the organism; they are interested merely in those facts which can be stated definitely in morphological and physiological terms, so that they can readily bring them into the laboratory for testing; they seldom deal with facts of heredity in the abstract. But there is a group of biologists, notably the eugenicists, who with most of the present-day psychologists insist that there is another kind of heredity, namely, the heredity of responses. It is this last notion of heredity that I wish to question in this paper; I have not the slightest intention of questioning the well-established results of recent Mendelian experiments on heredity, or even the superimposed theories so long as they are not uncritically applied to psychology; *what I am here concerned with is the problem of neuromuscular patterns—the physiomorphological basis of hereditary responses—and the problem of the mechanism of psychological heredity.* Unless they can describe all hereditary responses in physiomorphological terms, the behaviorists are not justified in talking about heredity in psychology at all, although the mentalistic and vitalistic psychologists can talk a great deal about it, because the latter are under no obligation to deal with objective and concrete facts, while the former are required to describe psychological phenomena in objective terms.

## I

## The Problem of the Relation of Behavior Pattern to Neuromuscular Pattern

By behavior pattern we mean the integration of separate bodily activities into an organismic adjustment. In phys-

iology—or better, in psychophysiology—we deal with such bodily activities as patterns of a lower order, namely, neuromuscular patterns; but in psychology proper we deal with them as an integrated whole, the organismic adjustment or behavior pattern. In other words, neuromuscular patterns are the material or elements out of which the behavior pattern is built. Thus, every organismic adjustment or behavior pattern can be analyzed into its elements, the neuromuscular patterns, although the properties of the former patterns are not inherent in the latter patterns. Now, the problem which confronts the student of objective psychology in connection with heredity can be stated thus: (1) *Are there any neuromuscular patterns corresponding to the supposed hereditary behavior patterns? And* (2) *assuming that there are definite neuromuscular patterns corresponding to the hereditary behavior patterns, how are they related to the germ-plasm?* I.E. *how are they correlated with the germinal organization?*

(1) Answering the first question, the inheritance psychologist of the laboratory type has a twofold task: (*A*) he must determine whether every behavior pattern has a definite, fixed, and invariable neuromuscular pattern, and if so, then (*B*) he must determine, locate and demonstrate such neuromuscular patterns. Until this twofold task is accomplished, he can not legitimately talk about heredity in psychology.

*A.* Recent studies in human and more particularly in animal behavior as well as in physiology have brought about a very definite and conclusive fact, namely, that a behavior pattern has no definite, fixed, and invariable neuromuscular patterns. Variability in the constituent elements of an organismic adjustment is a rule rather than an exception. The same behavior pattern of different individuals, or of the same individual at different times, may be made up of different movements, different receptors, effectors, and adjustors, while the same bodily mechanisms may be involved in different behavior patterns. Curiously enough, this fact has been conceded not only by the instinct deniers but by many instinct defenders as well (**4**, **9**, **12**, **13**). To show the complete

breakdown of the concept of non-variability of behavior pattern let me quote Dr. Tolman (**13**, p. 210–202), who has recently come out to defend strongly the concept of instinct. "It (the charge against the non-variability concept) calls attention to the extreme *flexibility* of most actual animal behavior. It asserts that nothing like real *reflex patterns* are to be found anywhere in nature. The solitary wasp, ammophilin, does not sting her caterpillars always in exactly the same degree of resultant paralyzation. Birds do not build their nests by means of a precise and invariable order of movements. Indeed these and countless like observations have given the pure *reflex pattern* theory its final *coup de grâce*." This is a really fatal indictment against the concept of hereditary responses in a type of psychology which always insists that every behavior pattern is analyzable into its physiological segments, and which maintains that nothing but physiomorphological features can be inherited. But for the sake of further discussion, let us waive for the moment this indictment and assume that every behavior pattern does always involve the same definite and fixed bodily mechanisms, so that we can immediately proceed to the consideration of the second task of our heredity psychologist, namely, that of locating and demonstrating the neuromuscular mechanisms for hereditary responses.

*B.* Though definite physiological correlates with hereditary responses have been assumed by a great many psychologists, no one has taken the trouble even to state, much less to show, what these correlates consist of. Watson, in his chapters on instinct (**14**, **15**), evades the whole problem with a vague definition that instinct is an hereditary pattern of reaction, the separate elements of which are movements principally of the striped muscles, and makes no attempt to show what movements and what striped muscles each instinct involves. When he defines the other hereditary mode of response, the so-called emotion, he does, however, seem to point out some definite sort of physiological mechanisms involved in the response, namely, the visceral and glandular systems. Other psychologists make great use of the concepts

of neural connections and synaptic resistance. According to them the neural connections of the hereditary responses are inborn and unlearned, while those of habits are acquired during lifetime. And, further, the synaptic resistance is low for hereditary responses; while new reactions have high resistance; learning or formation of habit consists, physiologically, in reducing synaptic resistance. Still others will assert that, viewed physiologically, hereditary responses are due to the predispositions of the nervous system. Let us examine carefully each of these various assertions.

(*a*) *Predisposition of the Nervous System.*—I really do not know just what is meant by such a vague phrase, nor can any one who has made use of this concept assure himself of its real implications. If by predisposition of the nervous system is meant the preformed arrangements of the system, then it is merely another name for the neural connection concept or the reflex pattern concept. On the other hand, if it refers to the readiness of the nervous system to respond to certain stimuli, it should be a very similar concept to that of synaptic resistance. Other than these two concepts, I have not been able to make out just what the expression 'predisposition of the nervous system' implies. We will take up the concepts of neural connections and of synaptic resistance in turn.

(*b*) *Neural Connections.*—Whenever this concept is used by the heredity psychologist, it always refers to the structural arrangement of the nervous system, the preformed or inborn pathways. It implies two things: first, that inherited pathways are open at birth, or before birth, or can function merely as a result of growth; and second, that learning consists in formation of new pathways. Although the evolution and development of the gross divisions of the nervous system in the embryo are more or less definitely known, modern embryologists have very little to tell us about the differentiation and development of particular reflex arcs in pre-natal life. At present we have no way of telling which paths are present at birth or before birth or merely as a result of growth, and which are formed as a result of learning. All this points

to the highly speculative character of the concept of neural connections in heredity. Aside from the question of actual evidence, there is a certain theoretical difficulty in this conception which has been pointed out by Watson (**15**, p. 272): "The conclusion is forced upon us that in habit no new elementary movements are needed. There are enough present at birth and more than will be combined into complex unitary acts. Since so many of the psychological texts speak freely of the formation of new pathways in habit it seems well to call attention to the simple mathematical fact that the number of permutations and combinations of, say, one hundred unit acts is a staggering number. Such speculations, though, are futile. One needs only to examine the five or six-day-old infant to be reasonably convinced that there is no need for the formation of additional reflex arcs to account for all later organization."

(*c*) *Synaptic Resistance.*—First of all it must be pointed out that the concept of synaptic resistance is little more than a hypothesis in neurology. We know very little about the nature of opposition in the hypothetical synaptic membranes. It is very questionable whether we should apply unproved theories of other sciences to psychology. Granted, however, that the theory of synaptic resistance is a well-established fact, we shall still ask: What has the theory to do with hereditary responses? In the first place, the theory implies only that there is a difference in threshold value between responses; high resistance means high threshold value and vice versa. This amounts to nothing more than saying that actions of low threshold can be called forth more readily than those of high threshold. On what ground, then, can we conclude that actions of lower threshold value are inherited responses while those of higher threshold value are not? Action *A* can be called out more readily than action *B* on account of the lower synaptic resistance of the former, so that action *A* is an inherited response. But why not call action *B* an inherited response also, since it is also a possible action and can certainly be called forth when the intensity of the stimulus is sufficiently increased? Secondly, the difference

in synaptic resistance between reflexes is merely a difference in degree. What, then, will be the degree of resistance which marks off an inherited reaction from a non-inherited one? I pray that those who have made great use of the synaptic resistance concept in explaining hereditary reactions may consider this question seriously.

(*d*) Finally, we come to the consideration of visceral and glandular organs as bases of inherited responses. We have pointed out that Watson has made a rather unsuccessful attempt to locate the so-called emotions in the visceral organs and glands. Other psychologists have recently made a great fetish of the endocrines. Due partly to failure of the reflex pattern theory in instinct and partly to some promising works on the function of the ductless glands, autacoids have become, for many of the present day psychologists, the *moving spirits* of the behaving organism. 'Pep,' 'drive,' 'driving adjustment,' 'determining tendency,' 'instincts,' 'emotions,' 'libido,' 'personality,' and what not, all can be located in the organs governed by the autonomous nervous system. Of course, we all more or less admit the importance of the functions of these organs which influence our behavior. But their importance should not delude us into thinking that they are the physiological basis of 'instincts' and 'emotions.' A little attention to the fact that the same glands may be involved not only in different instincts and emotions but also in a great many other activities, and that all the visceral organs and glands are in activity practically in every moment in our life, will convince one that differentiation between instincts and emotions cannot be made on the basis of these organs. This implies again that the supposed inherited reactions have no definite physiomorphological basis.

In passing, it may be noted that the autacoid substances, important as they are in behavior, act upon the organism as nothing more than intraorganic stimuli; they produce effects on the organism very similar to the effects of drugs. Feeding or injection of certain drugs will produce the same restlessness and uneasiness in the organism as do the internal secretions, and of course, no one will take such drugs as the source of

'instincts' or 'emotions'! Professor Tolman, in his article on the 'Nature of Instinct'(**13**) and in a recent correspondence with the writer, has pointed out the importance of the results of C. R. Moore's experiments on the testicular and ovarian transplantation (**10**). [In this connection see also an excellent historical summary by Stone (**11**).] Tolman says: "Such facts [Moore's results] certainly seem to demand some sort of instinct-hypothesis. The teleological hypothesis, with its allowance of more or less innate reflex patterns as required, would seem to satisfy this demand as well as any other." Professor Tolman does not seem to fully realize one serious implication of this conclusion. It implies that sex instincts can be exchanged between male and female, merely by exchanging the sexual glands. Well, then, what is an instinct? Shall we identify sex instincts with the sex glands? Allen and Doisy (**1**) injected active substances from the ovarian follicles of hogs and cattle into spayed animals and found that the injection caused typical œstrus changes. "During this artificially induced œstrum animals experience mating instincts, the female at times taking the initiative in the courtship, which culminates in copulation and the formation of a typical vaginal plug." Here we have an actual case of sexual reaction which can be produced by the injection of the active substance of the follicles. Will such a substance demand some sort of instinct hypothesis also? What is the teleology of such a substance? Moreover, the functions and growth of the sexual glands are also related to many other things than sexual reactions. The secretions of the Leydig cells may also produce other types of behavior than sexual intercourse. It is, therefore, physiologically incorrect to locate the sex instinct in the sexual glands. At any rate, personally I am inclined to think that the results of Moore's experiments as well as others tend to weaken rather than strengthen the instinct concept.

All the above discussions point to the urgent need for a psychophysiology. At present, we are more or less completely ignorant of the particular kinds and extent of the physiological apparatus involved in a given adjustment. Such igno-

rance offers the psychologists an opportunity to make use of general and vague theories, such as neural connections, synaptic resistance, internal secretions and what not, to explain general and ill-defined behavior categories. In this connection, I am in hearty agreement with Lashley (**8**, p. 351) that the chief handicap to the progress of behavior psychology is the lack of an adequate physiology. We need no longer delude ourselves, as did Watson and other behaviorists, into believing that the behaviorist with entire ignorance of physiological processes, can write an adequate description of behavior (**15**, p. 195). Just as biochemistry is essential to the progress of physiology, so is psychophysiology to the science of behavior. The general knowledge of the structures and functions of physiological apparatus is of little value to the psychologist; we need to know the particular reflex arcs, particular sense organs, muscles, and glands involved in each particular adjustment. We should endeavor to determine physiological facts as definite segments or components of behavior. We ought to have a technique of our own. We must not follow the example of the present-day psychologists, especially the inheritance psychologists, who borrow general and vague concepts from general physiology and merely use them as explanatory theories to conceal our ignorance of the real nature of physiological basis of behavior. We are interested in the actual facts of physiological correlations with particular reactions, rather than in the application of the general physiological concepts in our science. Let us cease to guess at any physiology of behavior until we can demonstrate it in the laboratory.

## II

### The Problem of the Mechanism of Psychological Inheritance

Even granted that the students of heredity in psychology have fulfilled all our requirements for a definite physiomorphological basis of hereditary responses, we will still have to inquire into the mechanism of such an inheritance. Are the neuromuscular patterns of hereditary responses preformed or

inherent in the germ-plasm? Just how does the germinal organization determine such patterns? Perhaps the problem of the mechanism of heredity has been a sufficiently difficult one even for the biologist. Most of the theories proposed by the biologists to solve the problem are highly speculative. The modified form of the preformation doctrine, namely, the doctrine of genes or the factorial theory, has been rather widely accepted by modern biologists to explain Mendelian heredity. But as a consequence of the acceptance of this theory the problem concerning the patterns and the chemical nature of the genes in correlation with the organismic pattern immediately arises, which problem probably cannot be solved until the much-dreamed of ultra-microscope is invented, which is powerful enough to study the structure, grouping, and behavior of the genes. In this connection it should be borne in mind that the most that both biometry and the method of experimental breeding can do for genetics is to bring out problems in heredity. Neither can solve any hereditary problem. The fundamental problem of heredity is the problem of mechanism. This problem can be solved only by cytology and developmental physiology. But at present these two sciences are not advanced enough to allay all our doubts about the validity of the new preformation theory. In fine, the present state of knowledge of the biologist concerning heredity and the germ-plasm is too meager to be of any use for the psychologist.

The above discussion has revealed the fact that heredity in psychology is not a fact, but merely an assumption. But of what value in the psychological laboratory is such an assumption? Is it not merely a great cloak devised by the psychologists to conceal their ignorance of the origin and development of behavior? At present, even the foundation of the science of psychology itself has not been firmly established; the fields of psychophysiology and developmental psychology have not as yet been touched upon. Why, then, assume so much? Why not go ahead in the laboratory and try to devise ways and means to study the developmental phases of behavior, together with their physiological correlates?

Certainly, it will not be too late for the psychologists to wait at least a few decades before they appeal to such ultra-microscopic gods for help; to wait until the psychophysiologist can locate definitely the neuromuscular patterns of each response and an ultra-microscope for the study of genes has been invented, or to wait until we have exhausted all the possible experimental methods of development of behavior as results of interactions between the organism and stimulations, intraorganic or extraorganic.

The fact is that if we assume heredity as an explanation of behavior we will have to explain, as already pointed out, various difficulties concerning the neuromuscular patterns and the cellular basis of hereditary responses; the explanation itself needs explanation. In brief, it does not explain behavior but simply explains away all problems in behavior in terms of heredity, and the problem of heredity remains.

The time seems to have come when we can no longer tolerate the tyrannic domination of biology in psychology; when we feel that there is need for a clear division of labor between biology and psychology, and that neither one should encroach upon the field of the other. Psychology as an independent science must have a system of its own, together with its own explanatory concepts. The geneticist is concerned with the problem of the origin and development of the organism; while *the psychologist takes the organism as given*, and investigates its adjustment relation with the environment. Behavior is always an interaction between the organism and its environment. Given an organism with its behavior history and given a stimulation, the psychologist has the task of determining the response. He needs the concept of heredity as much or as little as the concept of god. In fact, it makes very little difference to the psychologist whether the ultimate cause of behavior is heredity, nature, god, or soul, since heredity of behavior can never be proved as long as there is no one-to-one correlation in a fixed, definite, and invariable way between neuromuscular patterns and behavior patterns.

## III

### Concerning the Specific Types of Heredity in Psychology

*The Problem of Instinct.*—It is impossible at this time for me to attempt an extensive historical review of the arguments pro and con of the instinct concept during the last three years. What I wish to do here is (1) to amend some of the statements which I made in my earlier articles (**6**, **7**), and (2) to consider carefully the arguments in favor of instinct of those who have tried to attack the problem more or less from the laboratory point of view.

(*a*) *An Amendment.*—In my article on 'Giving Up Instincts in Psychology' (**6**) one of my main arguments against instinct is that all the so-called instincts are in the last analysis acquired responses. Such an argument would imply, in fact it admits, the traditional distinction between inherited and acquired responses, and would create certain difficulty when cases of unlearned responses are cited. Furthermore, such a distinction has led me to admit the inheritance of certain of the elementary responses out of which our complex reaction systems are built, namely, the units of reactions. This amounts to an abandonment of my original contention, for so long as there are inherited reactions, simple as they may be, there is justification for the use of the term instinct, although we may rightly object to its use for more complex responses. My confession is not that I had gone too far, but that I had made too much concession to the instinct psychologists, and had given them an open gap for attack. I gladly take this opportunity to amend my contention as follows: The traditional sharp distinction between inherited and acquired responses should be abolished. All responses must be looked upon as the direct result of stimulation, as interactions between the organism and its environment. We cannot attribute unlearned reactions to heredity, any more than we can so attribute other types of reactions. The units of reactions are no more inherited acts than are the complex habits of the adults. The problem of heredity is not a psy-

chological problem, since inheritance of psychological features can neither be proved nor disproved in the laboratory. This leads us to the considerations of the concepts of universality and non-acquisition as criteria of instincts.

(*b*) *Universality as Criterion.*—My contention is that universality of reaction is due either to universal organismic pattern or to universal environmental demands or to both. Upright walking is an universal response for the primates because they all possess legs and erect posture. Flight is universal in birds because they all possess wings. Food getting, respiration, rejection of waste products are universal and *inevitable* responses, to use Wells' terms (**17**), for every and all organisms including plants, *because of universal and inevitable organic demands. But does universality prove heredity? Just how is it related to the germ-plasm?* What has universality in behavior to do with the germinal organization? Wells (**17, 18**), while admitting that constant environmental conditions are partly responsible for the universal or inevitable responses, insists that such responses are dependent upon determiners transmitted through the germ-plasm. But what are such determiners and how are they transmitted through the germ-plasm? Are they the very determiners that determine the development of legs and erect posture in the case of upright walking, and wings in the case of flight? If so, it amounts to attributing universal responses to the universal organismic pattern. But of what value is it to distinguish on the basis of the universal organismic pattern between inherited and acquired responses? Furthermore, universality of organismic pattern does not necessarily mean the presence of universal reactions; it merely indicates the possibility of such reactions; whether they will actually appear or not depends entirely on whether or not there are *universal, constant,* and *inevitable* stimulating conditions. The emphasis is always upon the environment. In a word, while it is admittedly true that there are universal reactions due to fundamental and inevitable needs of the organisms, and due to the universality in their bodily make-up, the organismic pattern, their presence is no evidence for heredity, nor is

there any justification for making them the basis of distinction between habits and instincts.

(*c*) *Non-Acquisition as Criterion.*—The fact that there are reactions which can be performed without learning cannot be denied. But here again *does non-acquisition prove heredity?* Is non-acquisition also due to 'determiners, factors or genes transmitted through the germ-plasm'? Is there any correlation between the germinal organization and the unlearned behavior-pattern? If so, in what way? One may argue that non-acquisition is due to preformed nervous connections determined in the germ-plasm. We may briefly summarize the main points of our argument previously set forth against this contention: (1) There are no fixed and invariable nervous connections corresponding to an unlearned adjustment; (2) there is no embryological evidence to mark off the so-called inherited neural pathways from the acquired pathways; and (3) there is no actual evidence nor theoretical justification for the assertion that unlearned reactions are the result of any particular group of inborn neural pathways and that learning or habit-formation is a matter of establishing new pathways.

My second question concerning non-acquistion as a criterion of instinct is: What is the pragmatic or laboratory value in psychology which can be attributed to such a criterion? Suppose we observe three actions *A*, *B*, and *C*. Action *A* is *effectively* performed on the first trial, action *B* requires two or three trials in order to be *effectively* performed, and action *C* requires on hundred or more trials. On the basis of this criterion we should conclude that action *A* is inherited while actions *B* and *C* are acquired. Now, from this conclusion the danger and invalidity of the criterion can be readily seen. The difference in number of trials between actions *A* and *B* is only one or two trials whereas the difference between actions *B* and *C* is ninety-eight or more trials. And yet, in conformity with the criterion, actions *B* and *C* should be classified together as one type of reactions, the learned type as against the other type of reaction, action *A*, the unlearned type. Why is action *B* more akin to action *C* than

it is to action *A*? This I cannot really understand. My point is that for experimental purposes the distinction between reactions on the basis of non-acquisition is too crude to be of any value. As a matter of fact, actions do not just fall into these two opposite types, they vary in various degrees with respect to the ease, readiness, and rapidity with which they can be effectively performed. And so we should have a scale to measure the degree of relative ease and relative rapidity of acquisition of new reactions; such a scale should range from zero trials to hundreds or thousands of trials. Actions which can be performed without practice are no more instinctive acts than actions which require more than one hundred or one thousand trials; nor are actions with only two or three trials acquired responses any more than are the unlearned reactions. This is not intended as an *argument for the identity of instinct* and habit, but simply a plea *for a finer discrimination of reactions;* not to classify actions into two opposite groups, learned *vs.* unlearned, inherited *vs.* acquired, instinct *vs.* habit; but to have all actions fall in a scale so that the relative degree of ease and rapidity with which new reactions are acquired can be compared and studied.

When the relative degree of ease and rapidity with which new reactions can be effectively performed is compared, the psychologist immediately faces a genuine experimental problem: What are the factors which are responsible for the fact that one action can be acquired without any practice, another with two or three trials, another with ten trials, another with one hundred trials, etc? This is a very important problem in the study of acquisition of new reactions, and should be worked out in the laboratory of developmental psychology. But the concept of instinct with non-acquisition as its criterion has obscured this problem; in fact, such a problem will never appear under the concept of instinct. This is the main reason for my indictment of the view that instinct is a *finished* psychology, and I am still holding to the view which I expressed more than a year ago, that in the laboratory the instinct psychologist ends his investigation where the non-instinct psychologist begins (7).

*Tropisms, Reflexes, and Emotions.*—While the meaning of tropism and reflex can be definitely understood, the meaning of the so-called emotions is a very mooted question in psychology. For the present purpose, however, we may safely assume that the term emotion is used in psychology to designate a definite type of reaction which is assumed by many psychologists to be inherited through the germ-plasm. Now the chief argument for the inheritance of tropisms, reflexes, and emotions is also based upon the fact that they belong to the unlearned type of reactions. In the discussion of instinct we have presented our reasons for objecting to using non-acquisition as a criterion for determining hereditary responses. The same objections can be applied to the cases of reflex, emotion, and tropism. It is not necessary therefore for us to enter into any detailed argument against the notion that tropisms, reflexes, and emotions are inherited reactions.

The statement is often made by the inheritance psychologists that the organism, due to heredity, is so constituted that it will react tropismically or reflexively or emotionally under appropriate stimulation. Frankly, I have failed to grasp the significance of such a statement. Regarding this statement Kantor, whose chief attack is on emotion, has the following to say: "How informing is this statement? Such a statement is on a par with the assertion that the human individual is born to think, to perceive, to wear clothes, as well as to undergo various other experiences" (**5**).

*Mental Traits.*—When the study of heredity of morphological and physiological features began to be in vogue, a great many biologists and psychologists who were laboring under the delusion of the old-fashioned psychophysical parallelism thought that mental qualities must also be inherited and are inherited in the same manner as physical characters. Thus, one modern biologist dogmatically asserts, "There is no longer any question that some kinds of feeble-mindedness, epilepsy and insanity are inherited, and that there is often an hereditary basis for nervous and phlegmatic temperaments, for emotional, judicial, and calculating dispositions. Nor can it be denied that strength or weakness of will, a tendency

to moral obliquity, or rectitude, capacity or incapacity for the highest intellectual pursuits, occur frequently in certain families and appeared to be inherited" (**2**, p. 71). Working upon the assumption that mental inheritance goes parallel with physical inheritance, many biologists and psychologists, and more recently the mental testers, attempted many investigations of the heritability of individual differences, mental traits and racial characters. By juggling with statistical figures, many of these investigators conclude not only that mental traits are heritable but that many of them, such as insanity, feeble-mindedness, sexual crime, alchoholism and the like are inherited in Mendelian ratios. In this connection we must also mention the works by Yerkes (**19**) and Coburn (**2**) upon the heredity of wildness, savageness, and timidity in mice. The methods of these two investigators are relatively superior to those of Galton, Wood, Weeks, Goddard and others, in that while the latter used the historical and questionnaire methods, the former used the method of experimental breeding. But we are not concerned here with the reliability of the methods and of the sources of their information; for, even if we grant that all their information was gathered under strictly controlled conditions, we will still question the validity of their conclusions.

In the first place, are not all the so-called 'mental traits' merely terms for very vague and ill-defined catagories of social valuation of the organism's responses? What is insanity, what is sexual crime, feeble-mindedness, etc., and what constitute savageness, etc. in mice? Can any of the 'traits' be reduced to definite physiomorphological facts? It must be remembered that practically all of the so-called mental traits both in men and in animals are social names, each of which includes tens or even hundreds of different reactions, and each of these reactions may be, in turn, included in other categories of mental traits. And, what is more, the same bodily mechanisms of the organism may be used in combination with other mechanisms to produce actions which are found in every category of mental traits. All this points to the impossibility of locating definite physio-

morphological mechanisms of mental traits, and so we will be forced to speak of their heritability in the abstract sense, which sense is totally unacceptable to the objective psychologists.

Secondly, are the mental traits investigated by Galton, Wood, Goddard, Weeks, Rosanoff and others unit characters in the Mendelian sense? The Mendelian experiments deal strictly with definite morphological features, and it is sheer nonsense to speak of mental traits in terms of Mendelian ratio, when such traits are not reducible to definite physiomorphological facts.

## Summary and Conclusion

We may now summarize the main points of this paper.

1. The strictly behaviorist viewpoint in psychology is accepted by the writer as his theoretical foundation, upon which the whole discussion of the problem of heredity is based. So far as heredity is concerned, the behaviorist psychology has the following implications:

(*a*) That the problem of heredity should be considered strictly from the laboratory standpoint;

(*b*) And that since all facts of behavior are reducible to physiomorphological terms, and since biology does not deal with facts of heredity as abstract things, the behaviorist is required to state (*i.e.* to locate) hereditary responses not in general and vague, but in definite and exact physiomorphological terms.

2. But is is pointed out that behavior patterns do not usually have definite, fixed, and invariable neuromuscular patterns; and further, that the same physiological mechanisms may be employed in combination or in coöperation with other mechanisms to produce many varieties of reactions: the hands, the legs, the eyes, etc. are each and all used in innumerable ways to produce innumerable different reactions. This fact leads us to the conclusion that the neuromuscular patterns of hereditary responses cannot be definitely located in the organism in the same way in which the geneticist locates the color of hair or the shape and size of the eye,

or the length of garden peas. And so long as the same legs, the same hands, the same eyes, and ears, the same neurons, in short, the same physiological apparatus or organs are involved in all or many of the hereditary responses as well as of non-hereditary responses, such apparatus cannot be taken as hereditary units for any given responses, and psychological inheritance becomes a purely metaphysical abstraction.

3. The validity of the general concepts of neural connections, of synaptic resistance, of predisposition of the nervous system, and of internal secretions as applied to the problem of hereditary responses, has been seriously questioned in this paper.

4. The assumption of heredity is a stumbling-block in the way of the progress of experimental psychophysiology and developmental psychology. The inheritance psychologists try to solve the problem of the origin and development of behavior by postulating the existence of hereditary factors of a very mystical and non-experimental nature, and thus substitute a metaphysical problem for the scientific and laboratory problem.

5. The validity of universality and non-acquisition as criteria for measuring instincts and other modes of hereditary responses has also been questioned.

6. The so-called mental traits in men and animals are very vague and ill-defined socialized behavior categories, each of which includes a group of many different reactions. Hence no mental trait has any definite physiomorphological constituents which are not at the same time constituents of other traits.

From the above facts the conclusion is forced upon us that unless we are willing to accept the vitalistic or the mentalistic program in psychology, in which program heredity can be justly dealt with in the abstract, the entire concept of heredity should be dismissed from our science.

At present, we do not need so much the assumption of heredity in psychology as we need experimental technique for the study of psychophysiology and developmental psy-

chology. The success of behaviorism depends to a very great extent upon the success of these two branches of the science. The behaviorists have accepted the concept of heredity from the traditional psychology without realizing that such a concept is a lazy substitute for energetic and painstaking work in experimental psychophysiology and developmental psychology. Indeed, unless we are willing to admit that, judging from the tenor of recent publications, the whole behaviorist movement will amount merely to a rephrasing of the older psychological categories, re-interpretation of consciousness, re-interpretations of sensation, perception, emotion, and the like, unless we want to be content with pure speculations and unprovable assumptions, unless we do not want to build a constructive program of behavior psychology upon the basis of experimental results of psychophysiology and developmental psychology, I earnestly invite the attention of the behaviorists to the serious consequences of the application of those general and vague concepts of general physiology to our science and of the assumption of the heredity of responses.

## References


1. Allen, E. Racial and Familial Cyclic Inheritance, etc. *Amer. J. Anat.*, 1923, **32**, p. 301.
2. Coburn, Ch. A. Heredity of Wildness and Savageness in Mice. *Behavior. Monog.*, 1922, **4**, p. 71.
3. Conklin, E. G. Heredity and Environment. Princeton University Press, 1919, p. 71.
4. Hocking, W. E. The Dilemma in the Conception of Instincts as Applied to Human Psychology. *J. Abn. Psychol.*, 1921, **16**, 73–96.
5. Kantor, J. R. An Attempt toward a Naturalistic Description of Emotions. Psychol. Rev., 1921, **28**, p. 37.
6. Kuo, Z. Y. Giving up Instincts in Psychology. *J. Phil.*, 1921, **29**, 645–666.
7. Kuo, Z. Y. How are Our Instincts Acquired? Psychol. Rev., 1922, **29**, 344–365.
8. Lashley, K. S. The Behavioristic Interpretation of Consciousness. Psychol. Rev., 1923, **30**, p. 351.
9. McDougall, W. The Use and Abuse of Instinct in Social Psychology. *J. Abn. Psychol.*, 1921–1922, **16**, 285–333.
10. Moore, C. R. On the Physiological Properties of the Gonads as Controllers of Somatic and Psychical Characteristics. *J. Exp. Zool.*, 191–, **28**, 137–160, 459–467; 1921, **33**, 129–172, 365–390.
11. Stone, C. P. Experimental Studies of Two Important Factors Underlying Masculine Behavior, etc. J. Exp. Psychol., 1923, **6**, 85–106.
12. Tolman, E. C. Can Instincts Be Given Up in Psychology? *J. Abn. Psychol.*, 1922, **17**, 139–152.

13. TOLMAN, E. C. The Nature of Instinct. PSYCHOL. BULL., 1923, 20, 200–218.
14. WATSON, J. B. Behavior, Henry Holt, 1914, Chap. 4.
15. WATSON, J. B. Psychology from the Standpoint of a Behaviorist. Lippincott, 1919, Chaps. 6 and 7.
16. WATSON, J. B. Is Thinking Merely The Action of the Language Mechanisms? *Brit. J. Psychol.*, 1921, 11, 87–104.
17. WELLS, W. R. The Meaning of 'Inherited' and 'Acquired' in Reference to Instinct. *J. Abn. Psychol.*, 1922, 17, 153–161.
18. WELLS, W. R. The Value for Social Psychology of the Concept of Instinct. *J. Abn. Psychol.*, 1922, 16, 334–343.
19. YERKES, R. M. The Heredity of Savageness and Wildness in Rats. *J. Animal Behavior*, 1913, 3, 286–296.

# STIMULATION RANGES AND REACTION AREAS

BY FRANKLIN H. GIDDINGS

*Columbia University*

Behavior of any description, animal or human, individual, social or societal, is a product of five generic factors, each of them divisible and subdivisible as far as you please.

The first factor is a sustentation field: an inhabitable bit of the earth's surface capable of producing food, and otherwise of providing for an upkeep of plant and animal life. The second factor is an ancestry of commingled dominant and recessive traits, which has handed down in heredity a mechanism of 'original nature.' The third factor is a certain range (comprising a reach and a scatter) of stimulation. Some stimuli travel far, others but a little way. The fourth factor is an extent or area of reaction. Not all neurons and not all individuals reached and hit by a given stimulus respond to it. Those that do respond make up a reaction area. The fifth factor is a history of primary conditionings and successive reconditionings of reflexes and their combinations.

In the present brief paper I am attempting only to call attention to stimulation ranges and reaction areas and their significance. We have here, I think, related facts of some importance, which have not received adequate consideration.

A stimulus may reach and impinge upon only a peripheral spot of an individual organism. A needle prick is a familiar example. It disturbs only a minute net or nest of neurons. Another stimulus, for example, a bucketful of cold water, may within an instant hit the entire periphery of one individual organism, but of one only. Intermediate in range between these stimuli are such stimuli as a limited surface burn, the slap of a hand, a 'draft' blowing on one's neck and the back of one's head.

A stimulus of greater dispersion normally reaches all, or nearly all, of the individuals composing a small company or

group. The whirr of a partridge, the warning of a rattler, the start of a rabbit, normally reaches the ears or eyes of each of four or five boys or girls in close formation on a hike. The glow of hearth fires, the savory odors of dinner in preparation, normally reach all members of a household gathered within its domicile. The range of these stimuli is the radius of a relatively intimate group.

Of considerably longer reach and wider scatter are the blast of a mill whistle, the clang of the bell or the shriek of the siren of a fire engine or an ambulance, the glare of a conflagration, the crack of a rifle, the booming of guns, the harangue of a street orator. These reach a multitude, or throng.

Finally, there are such stimuli as weather, climate, and the fertility of a wide stretch of land; as possibilities presented by forests and by deposits of iron, coal, or oil; and as opportunities offered by harbors, and by navigable rivers. These reach and scatter throughout considerable distances. Their range is an entire population, composed of intimate groups, families, hamlets, and scattered homesteads; and of miscellaneous multitudes and crowds.

The range of stimulation appears to be of wide variability. It is determined by physical facts, the nature and arrangement of which need not now concern us. Let us turn then to reaction areas.

Not every neuron reached by the needle reacts to it continuously, or every time. Much less do all the neurons reached by the stimulation set up by the bucketful of cold water so react. Not every boy or girl in the company of hikers invariably starts at the whirr of the partridge or the sound of the rattler. Not every member of a domestic group is attentive to every household incident. Not everybody in a neighborhood responds to the mill whistle, nor does every boy in the street run after the fire engine. Scores of individuals saunter by the soap-box orator without stopping to listen to him. No habitable region, however advantageous, holds all prospectors as settlers.

The extent of a reaction area is determined by factors

which, unlike the facts that determine range of stimulation and which we have passed by, are significant for our present purposes.

One is the intensity and amount of the stimulation. Within limits not strictly determined, increasing intensity of stimulation calls into reaction an increasing number of neurons. In like manner, increasing strength of any stimulation which is of sufficient range to reach respectively all members of an intimate group, or of a multitude, or of an entire population, calls into reaction within limits which as yet are only very roughly determined an increasing proportion of them. The spring of a wild cat would startle every member of our hiking party. A terrific explosion, or the collapse of a public building would arrest, at least momentarily, every individual of a multitude within stimulation range. An earthquake, a volcanic eruption, or a military invasion would call forth reactions of consternation in all, or approximately all, individuals composing a dense and widespread population.

The second factor determinative of reaction area is the relative homogeneity or the relative heterogeneity (as one or the other word may the better describe a given state of facts) of the units composing the *plurel*[1] potentially reactive. Presumably neurons are not precisely alike in molecular structure. Individual organisms certainly are not altogether alike, and among differences we have such relatively big ones as those which constitute sexes, age, classes, constitutional differences, and differences of race. A population made up of whites of various nationalities, and of yellow, red, and black stocks of numerous varieties, cannot as uniformly react, even to stimuli of great strength, as can a population relatively homogeneous in these particulars.

The stimuli of great range which determine the activities of human populations throughout long periods of time are assembled and arranged in groupings which collectively we call 'circumstances.' At all times these condition and play upon human life. In the aggregate they constitute a varying

[1] This is a useful archaic substantive corresponding to the adjective 'plural.'

but never ceasing pressure. Circumstantial pressure, then, and the degree of homogeneity of a population are the two prime variables which determine the structure and activities of human society, and the course of human history.[1]

Have we not now found significant differentiæ which broadly mark out divisions of psychology which it has become convenient, if not indeed necessary, to recognize for purposes of intensive scrutiny? Stimuli of such limited range that they reach only one individual at a time and disturb only a correspondingly limited reaction area give us the distinctive or differential phenomena (but not all the phenomena) of individual psychology. Stimuli of greater but nevertheless limited range which reach all the individuals of an intimate group or company (companions, *socii*) and the corresponding reaction area give us the differential phenomena of social psychology. Stimuli of considerable range which reach a multitude of individuals and the corresponding reaction area give us the differential phenomena of crowd psychology. And finally, stimuli of indefinite range which reach all the intimate groups and multitudes that compose and constitute a population, and a correspondingly wide reaction area, give us the differential phenomena of societal psychology.

Of the countless reconditionings whereby the reactions of populations to stimulations of indefinite reach and bewildering scatter are fashioned into human society only brief and highly generalized descriptions can be offered at this time. Enumerated in order of occurrence they are: conditioning by interstimulation and response, reconditioning by kind, reconditioning by speech, reconditioning by spoken discriminations of kind (subjectively the 'consciousness of kind'), and reconditioning by integrations of habit (folk-ways, culture). These reconditionings enter into an integration of co-individual behavior.

In the beginning is multi-individual response (*i.e.* responses by many individuals) to one after another wide-ranging stimulation. There is congregation at places: feeding places, drinking places, places that offer shelter and security. There is concourse and commingling on occasion, when things

[1] Giddings, 'Studies in the Theory of Human Society,' pp. 252 and 257.

happen. Multi-individual responses may be simultaneous or approximately so, or they may occur with varying degrees of promptness, differences from which emerge leadership and following. They may be prevailingly alike or prevailingly unlike, and herein lie all possibilities of conflict, competition, and coöperation.

Conditioning begins with interstimulation and reactions to it. In any aggregation or assemblage of animals or of human beings the behavior of each individual is stimulus to many of his fellows, now and then to all of them; and some of them, now and then all of them, react. With this interchange communication begins, and herein lie the possibilities of suggestion and suggestibility, of example, imitation and mass intimidation. An exceedingly important phase of multi-individual behavior conditioned by interstimulation is dramatization. In the presence of another or of others the acts of any and each individual become acting. Conditioned by interstimulation and response multi-individual behavior becomes co-individual behavior.

Reconditioning begins with facts and distinctions of kind. Creatures of one identical kind or variety tend to keep together and to go together. This is not because of gregarious instinct.[1] It is because, first, offspring of the same parents and often most of the offspring of one ancestral line for two or three generations hold together by inertia unless an extraneous cause scatters them; and, because, second, holding together is the line of least resistance. Creatures of identical kind do not as a rule repel one another, partly because they are not usually as dangerous to one another as creatures of unlike kind are, and partly because the cries and other behavior of similars are in everything except individual source almost identical with the auto-stimulating behavior of each individual provocative of reactions within himself.

That the fact last alleged is more substantial than a mere ingenious assumption might be, will be conceded, I think, if we reflect upon the certainty that without the similarity

[1] I do not admit that there is a gregarious instinct. My reasons for denying it are presented in a chapter on 'The Mind of the Many' in my 'Studies in the Theory of Human Society.'

but never ceasing pressure. Circumstantial pressure, then, and the degree of homogeneity of a population are the two prime variables which determine the structure and activities of human society, and the course of human history.[1]

Have we not now found significant differentiæ which broadly mark out divisions of psychology which it has become convenient, if not indeed necessary, to recognize for purposes of intensive scrutiny? Stimuli of such limited range that they reach only one individual at a time and disturb only a correspondingly limited reaction area give us the distinctive or differential phenomena (but not all the phenomena) of individual psychology. Stimuli of greater but nevertheless limited range which reach all the individuals of an intimate group or company (companions, *socii*) and the corresponding reaction area give us the differential phenomena of social psychology. Stimuli of considerable range which reach a multitude of individuals and the corresponding reaction area give us the differential phenomena of crowd psychology. And finally, stimuli of indefinite range which reach all the intimate groups and multitudes that compose and constitute a population, and a correspondingly wide reaction area, give us the differential phenomena of societal psychology.

Of the countless reconditionings whereby the reactions of populations to stimulations of indefinite reach and bewildering scatter are fashioned into human society only brief and highly generalized descriptions can be offered at this time. Enumerated in order of occurrence they are: conditioning by interstimulation and response, reconditioning by kind, reconditioning by speech, reconditioning by spoken discriminations of kind (subjectively the 'consciousness of kind'), and reconditioning by integrations of habit (folk-ways, culture). These reconditionings enter into an integration of co-individual behavior.

In the beginning is multi-individual response (*i.e.* responses by many individuals) to one after another wide-ranging stimulation. There is congregation at places: feeding places, drinking places, places that offer shelter and security. There is concourse and commingling on occasion, when things

[1] Giddings, 'Studies in the Theory of Human Society,' pp. 252 and 257.

happen. Multi-individual responses may be simultaneous or approximately so, or they may occur with varying degrees of promptness, differences from which emerge leadership and following. They may be prevailingly alike or prevailingly unlike, and herein lie all possibilities of conflict, competition, and coöperation.

Conditioning begins with interstimulation and reactions to it. In any aggregation or assemblage of animals or of human beings the behavior of each individual is stimulus to many of his fellows, now and then to all of them; and some of them, now and then all of them, react. With this interchange communication begins, and herein lie the possibilities of suggestion and suggestibility, of example, imitation and mass intimidation. An exceedingly important phase of multi-individual behavior conditioned by interstimulation is dramatization. In the presence of another or of others the acts of any and each individual become acting. Conditioned by interstimulation and response multi-individual behavior becomes co-individual behavior.

Reconditioning begins with facts and distinctions of kind. Creatures of one identical kind or variety tend to keep together and to go together. This is not because of gregarious instinct.[1] It is because, first, offspring of the same parents and often most of the offspring of one ancestral line for two or three generations hold together by inertia unless an extraneous cause scatters them; and, because, second, holding together is the line of least resistance. Creatures of identical kind do not as a rule repel one another, partly because they are not usually as dangerous to one another as creatures of unlike kind are, and partly because the cries and other behavior of similars are in everything except individual source almost identical with the auto-stimulating behavior of each individual provocative of reactions within himself.

That the fact last alleged is more substantial than a mere ingenious assumption might be, will be conceded, I think, if we reflect upon the certainty that without the similarity

[1] I do not admit that there is a gregarious instinct. My reasons for denying it are presented in a chapter on 'The Mind of the Many' in my 'Studies in the Theory of Human Society.'

of stimulation by kind to auto-stimulation, speech could not have been acquired. For, as a behavioristic fact, speech is precisely such an approximate identity of self and other stimulation, of self and other response. With the acquisition of speech a further reconditioning of co-individual behavior began, more radical and far-reaching, perhaps, than any other in the whole history of the human race. It brought every phase of the experience of each individual to bear upon the behavior of every other, and it made possible the handing on of experience, and of acquisitions too subtle for transmission through any other medium, from generation to generation. Conversationalized experience became knowledge, an essential part of which was conversationalized discrimination.

With discriminations talked about came sortings, the beginnings of classification, of distinctions of kind; and among these the most important by far was a talked about discrimination of 'own kind' from 'other kind,' of 'my kind' and 'our kind' from 'your kind,' 'his kind,' and 'their kind.' Without entering into the question upon which behaviorists and the psychologists of subjectivism are at odds let us say that in the language of the vulgar (if we are talking as behaviorists) or of the esoteric (if we are subjectivists) the phenomenon of which we now speak is an 'awareness' or 'consciousness' of kind. When men attained it they began to be *social* as already they had been *gregarious*. Now they not only *consorted* by kind, but also they began to *associate*, picking and choosing companions and confirming their likes and dislikes by talking about them. It was, in short, the 'consciousness of kind,' or at any rate, the 'talking about' distinctions of kind *that converted the animal herd into human society*, a reconditioning of all behavior second in its tremendous importance only to the effects of speech itself.

Finally, came reconditioning by an integration of habits and an accumulation of knowledge (both of which now were talked about) which had been made possible by speech and association. The integrations constituted folk-ways or customs, and the acquisitions became cultures and culture

patterns. The reconditionings which these have brought about constitute our so-called civilization.

These conditionings and reconditionings of co-individual behavior have not, of course, appeared one after another only to disappear. Each has supplemented and reacted upon whatever went before. By means of them (co-effective always with primal multi-individual response to wide-ranging stimulation) co-individual behavior has been integrated and fashioned into the fabric of human society.

# PSYCHOLOGICAL AND SOCIOLOGICAL TYPES

BY HEINRICH KLÜVER

*Stanford University*

Ten years ago C. G. Jung drew attention to the importance of types, in the following words (quoted in his 'Collected Papers on Analytical Psychology'): "I must emphasize the statement that this question of types is the question of our psychology, and that every further advance must probably proceed by way of this question." One may doubt whether 'every further advance' since that time has really taken place in this way; but one cannot doubt that in recent discussions this problem has played a part of great importance. For example, at the last meeting of the British Association for the Advancement of Science, held in September, 1923, Cyril Burt, in a Presidential Address entitled, 'The Mental Differences between Individuals,' was concerned with certain present-day classifications of mental types. O. Selz and R. Sommer dealt with the same problem at the last German Kongress für experimentelle Psychologie.

It is not only psychoanalysts who recognize the importance of the investigation of types. In recent years an approach has been made from various angles. Classifications of types have been worked out in psychiatry, pedagogy, and psychoanalysis, on the basis of Dilthey's '*Struktur*' psychology and from the point of view of a 'phenomenological psychology.' There should be mentioned in this connection beside Jung's 'Psychological Types,' Ernst Kretschmer's '*Körperbau und Charakter, Untersuchungen zum Konstitutionsproblem und zur Lehre von den Temperamenten*,' H. Rorschach's '*Psychodiagnostik*,' E. Spranger's '*Lebensformen*,' K. Jasper's '*Psychologie der Weltanschaungen*,' R. Müller-Freienfels' '*Persönlichkeit und Weltanschauung*' and finally the experimental investigations of the Marburg school (E. R. Jaensch and W. Jaensch) on '*Eidetiker*.'

It is not my intention to give a summary of these classifications of types established by means of different methods, nor do I plan a discussion of the difficulties involved in the concept 'type.' The problem that concerns me here is: What relation exists between 'psychological' and 'sociological' types?

Even a very superficial study reveals the fact that the so-called 'psychological' types are not always types in the sense that their constituents are purely psychological. William Stern pointed out in '*Die differentielle Psychologie in ihren methodischen Grundlagen*' (1911) that a sharp distinction must be drawn between 'phenomenological' and 'psychological' types. Qualitatively different reactions to a phenomenologically given material may be observed as in the description of a picture, or a cigarette (*Binet*) or a statuette (*Nogrady*). It is thus possible in certain circumstances to distinguish phenomenological types, but these are still a long way from real psychological types. The same is true for the relation between 'sociological' and 'psychological' types, they are not coincident.

I purpose to discuss the problems involved here, in connection with Arthur Kronfeld's '*Das Wesen der psychiatrischen Erkenntnis*,' for this study will throw light upon certain obscurities of our problem.

According to Kronfeld, every psychopathic type designates an inductively discovered real unity, the representation of which is possible only in the concrete. For clinical investigations, one must make use of descriptively determinable data and of raw investigations made to fit the purpose. By proceeding in this way one remains within the reach of descriptive psychology.

Criminology and considerations of a criminological nature show clearly that besides the psychological and descriptive, the psychiatrist has also to take into consideration sociological points of view. Criminology deals with the different types of antisocial behavior. The term 'antisocial' is in most cases determined by the norms of the penal codification. The necessity arises of bridging over the gap between the *legally*

determined *crime* and the *psychologically* determined structure of the *person* who committed the crime. The central question may be stated thus: May social behavior be accepted as a criterion for the determination of psychological (or psychopathic) types?

To begin with, the objection might be raised that the well-known types of psychopathic personalities are mere abstractions. They are abstractions, but they are so in the same way as the 'normal person' is an abstraction. If one begins to establish types, one is seeking for a formula that expresses the interaction of mental functions in a given individual. Such a formula cannot be stated explicitly, but having different types, we know that the interrelation of the different functions and the way they act together is different in each type. All that can be done is to determine certain clinical groups, *i.e.* to give a descriptive enumeration of those phenomena that are presumably important in a genetic-etiological respect. But in putting together similar observable states and processes, oversimplification must be avoided. If the observable states of different individuals seem to indicate differences of function, they cannot, of course, be considered as belonging to the same type.

The way in which psychopathic types are established is not especially important. But once having defined the types, it will be much easier to understand the irregularities of the social behavior characterizing each. Again, the problem is further complicated. In order to determine psychopathic types, we not only base our classification on mere psychological observations, but we also make use to a very great extent of reports on the social conduct of the individual. Since deviations from the social norm are not always deviations from the psychological norm, the question as to method arises: To what extent can statements that concern social behavior be used in establishing types? As an illustration one may take Prichard's 'moral insanity.' The concept of 'moral insanity' necessarily leads into an impenetrable wilderness of anthropological, psychological, psychiatric, penal, ethical, and metaphysical questions. Kronfeld's opinion is

that *if* in the case of moral insanity we have to do with a psychologically uniform type, this type must be definitely circumscribed; *if*, however, we have to do with the criminological-practical type of the incorrigible, the chronic criminal, this type must be analyzed into its basal psychological types. In taking into consideration the works of Aschaffenburg, Berze, Liepmann, and Bleuler, Kronfeld tries to show the psychological meaning involved in this concept of 'moral insanity.' His conclusion is as follows: Behind the social behavior of man, the constant effect of the same functions and dispositions is not always operative; therefore, antisocial conduct is not the effect of a constant psychological disturbance of these functions and dispositions. Moreover, the environmental conditions are ever changing; it is therefore impossible to characterize once for all the reactions of even a pure psychological type. In these circumstances, statements concerning the 'antisocial behavior' of the *reo nato* cannot mean very much if the *psychological* nature of the type is under consideration. Expressions of such a nature are useless for any classification of psychic structures. Identical motives may lead to entirely different actions; the same antisocial action may, indeed, rest upon psychologically different foundations. If we arrange the types as to their social behavior, we do, in fact, get *types of action*. We take the social *value* of the action into consideration, and we see at once this social origin in concepts such as 'affective inferiority,' 'psychopathic inferiority,' 'unstable individual.' The types of action (Tattypen) established by criminal sociology are incidental so far as psychology is concerned; they are constructed by a dogmatic application of the existing criminal law, the nature of which is partly determined by the political and economic conditions of the particular society in question. By reviewing social behavior on the basis of the norms of penal law which have been negatively determined and are neither psychologically nor socially uniform, we distinguish *criminological* types. Through observation we are led to specific mental structures, to psychological types.

What then is the relation between criminology and the psychology of crime? First of all, starting from criminological

types, one might try to investigate and compare different mental causes and motivations, in order to determine to what extent acts are the bases for the mental development of those who perform them. Or, proceeding from a descriptive classification of psychopathic types, one might examine their peculiarities with reference to the social environment. But the recent literature of the psychology of crime shows the inadequacy of these two approaches. There is almost no hope of removing the difficulties encountered here. Is a solution impossible?

Kronfeld finds a solution by way of introducing the concept of 'reactivity' (*Reaktivität*), a specific psychic interrelationship of functions. He starts from the consideration of whether or not it is necessary for the determination of descriptive-psychopathic types to make use of social criteria *besides* the direct psychic characteristics. In a descriptive sense, it is undoubtedly necessary. The foregoing discussion, however, dealt with social criteria in a *normative* sense. In order to characterize certain types, one has to take into consideration the social behavior from a purely descriptive point of view. This must be done, for example, in the case of the erethistic imbecile. A certain group of social actions has to be considered as an expression of a special 'reactivity' of a psychological type; on the other hand, an adequate characterization of this 'reactivity' is only possible by means of a *description* of certain social actions. Any *normative* evaluation is omitted. To illustrate different kinds of '*Reaktivität*,' Homburger (*'Lebenschicksale geisteskranker Strafgefangener'*) names the 'pathological swindler,' the hysteric-eccentric and paranoiac types, and the persons characterized by constitutional depression.

In what way does this 'reactivity' furnish a solution of our problem? We pass from social actions to the agency of certain mental functions. This does not imply that we are always clear about the exact *interrelationship* of these functions. A special 'reactivity' is found whenever we are forced to assume the agency of certain functions, because, as observation shows, some characteristic behavior or way of

acting always occurs. Thus, the directly observable social behavior is the only index of this functional basis.

It is now clear that, for example, in the case of the hysteric-eccentric type, one refers *to a certain extent* to the interrelationship of its psychic functions. It is clear, moreover, that a recognition and description of this type is impossible without reference to social behavior. On the other hand, it goes without saying that the concept of the 'born criminal' cannot be traced back to a group of psychic functions bound up in one definite way. A closer analysis makes evident the heterogeneous types involved.

We must keep in mind that Kronfeld's solution is a solution for the psychology of crime. Social behavior means the expression of a uniform psychological 'reactivity.' Now, let us not fail to recall the fact that in a concrete case the determination of those traits of the social behavior which have to be considered as the result of certain psychological factors is extremely difficult. There are, *e.g.* hysteric reactions which are not the reactions of hysteric-eccentric individuals. In spite of these difficulties, the concept 'reactivity' forms in a rough way the connecting link between 'psychological' and 'sociological' types. It is conceivable that a specific 'reactivity,' a specific psychic interrelationship of functions, leads to a specific social behavior. Moreover, it is clear that a different social environment changes the mode of reaction. It may be characteristic for *milieu* A to cultivate certain psychopathic types; that means, that this particular *milieu* A permits a cruder means of expression for a certain type than another *milieu* B. In fact, only the 'expressions' change, the psychic functions involved are the same.

It is obvious that Kronfeld's *criminal psychological* types are not psychological types. If we observe very often an inadequacy between stimulus and reaction and thus construct an eccentric type, we still have *in a certain sense* a 'phenomenological' and not a psychological type. It is, moreover, not feasible to use a compact concept of *milieu*, which is nothing more than a product of economic, social, and legal factors. Every individual has his environment. What is

called *milieu* must be broken up into *milieux*, each of which is—to adopt a saying of Zola—'coin de la nature vu par un type.' Only within a *milieu* of such a character pertaining to a special type can one distinguish reactions with reference to quantitative and qualitative aspects. But it is not my purpose to investigate here, how in face of these variations of types and *milieux*, it might still be possible to furnish an objective basis for the classification of types.

# THE NATURE OF SUGGESTIBILITY

BY JOHN J. B. MORGAN

*The State University of Iowa*

## Introduction

Certain experiments performed in the laboratory of the Psychological Clinic of the State University of Iowa [1] in the last few years have served to throw some light on the nature of suggestibility and negativism, to indicate the connection of these phenomena with other psychic processes and to distinguish certain types of personality with such clearness that it seems advisable to give a general discussion of the theoretical implications of this work.

Suggestibility and negativism have been used with no clear notion of what these terms connote. The meanings have ranged from sheer mysticism to absolute mechanism, but in spite of this range of definitions the phenomenon of suggestibility has been known from time immemorial.

Parmelee,[2] giving expression to the mechanistic view, defines the term as 'the process by which a stimulus is transmitted from a receptor center by means of an association center to a motor center' and states that "the suggestibility of an individual depends upon the ease with which such transmission takes place within the central nervous system." According to this concept one is suggestible when any situation finds a ready formed pathway leading to a definite discharge.

If this view is held, suggestion is enhanced by any mental process which tends to facilitate associations. Learning to play the piano makes one suggestible to the music score. A mere glance will set off numerous responses of a very definite sort. The difficulty with this notion is that the opposite type of performance—negativism—is merely synonymous

[1] Travis, Lee E., 'Studies in Dissociation.' J. Exper. Psychol., 5, Oct., 1922.

Travis, Lee E., 'Measurement of Suggestibility and Negativism.' *J. Abn. & Social Psychol.*, 19, 1924.

[2] 'The Science of Human Behavior,' Macmillan, 1913, p. 247.

with unlearnedness. One is suggestible when certain reaction patterns are set off by the music score; one is negativistic when he is untrained so that the music score produces no such response. Negativism is ordinarily not used in this manner. We would not say a person was negativistic if he could not play, but we would say he was if he could play and refused to do so or if he refused to try to learn when he had no good reason (according to our viewpoint) to refuse the suggestion.

Janet, on the other hand, regards suggestibility as an abnormal phenomenon and proposes it as the 'most fundamental stigmata of the hysterical state.'[1] He tries to distinguish between the ordinary response to an external impression and the response of a suggestible person. The ordinary individual when given an idea has other ideas follow in the train of this first one, but the 'images are always dim,' the tendencies to action are always vague and slight, the idea develops into action only with the coöperation of an additional volitional element, the personality must back the idea and the end reaction is accompanied by conscious effort. In the suggestible person, on the other hand, a clear-cut idea "seems to be transformed and to become at once another psychological phenomenon, an act or a perception. In fact, they almost immediately move their limbs in a manner quite visible outwardly. They really get up and dance; they walk, run, jump, struggle, cry." "Each idea seems to develop to the maximum, to give all it contains in the way of images, muscular movements, and visceral phenomena. This complete development of all the elements contained in an idea is an essential characteristic of the phenomenon."[2]

Suggestibility, in our opinion, involves more than ease of conduction in the sense of Parmelee's definition. Ease of conduction is an element, but it is not a sufficient explanation of suggestibility to identify it with associational processes. Nor can we agree with Janet that it is merely an immediate and complete response to an idea. We believe that *suggestibility is an attitude or set which makes a person amenable to a wide*

[1] 'Major Symptoms of Hysteria,' Macmillan, 1913, p. 279.

[2] *Ibid.*, p. 282.

*range of stimulus situations.* It is this set which makes associational processes easy, which makes possible immediate and complete responses to stimuli. It is our purpose to analyze the factors involved in this set, to show how such a set develops, how it influences a person's conduct, to what abnormal degrees it may go and to indicate how it may be measured by laboratory methods.

## Factors Involved in Suggestibility

What strikes us particularly about suggestion is that the response is very likely to be out of all proportion to the strength of the stimulating situation. If the response were always directly proportional to the stimulus, there would be no occasion for the term suggestion. But this paradox is not confined to complex behavior. We know that even in reflex responses there is little general correspondence between the energy of the stimulus and that of the response. Some reflexes show a powerful response to a weak stimulus, while others give a feeble response to a strong stimulus. This situation is especially apparent in the so-called spread of a stimulus. As a stimulus becomes stronger the response becomes more and more widespread until the 'irradiation' may involve the larger part of the organism. In all this spreading only those muscles are involved which lead to a harmonious result. The nature of this spreading has been clearly set forth by the researches of Exner [1] and Sherrington.[2] They showed that two stimuli acting simultaneously can either lead to a combined harmonious result or they can antagonize each other and lead to a conflicting result. The former situation is related to the simple spreading caused by increasing the strength of a single stimulus and was called by them facilitation. The latter situation they called inhibition.

There is good reason to believe that these phenomena of facilitation and inhibition apply to more complex mental processes as well as to the simple reflexes. It is quite possible to have a situation which would lead to a certain complex form of behavior in the normal organism. In the face of

[1] *Pflüger's Arch. f. d. ges. Physiol.*, 1882, **28**, 487.

[2] 'Integrative Action of the Nervous System,' Scribners, 1906, pp. 114–234.

such a situation it is possible to have an organism tuned to respond in the same way that the situation dictates. Here the inner set and the outward stimuli reinforce each other and you have the phenomenon of facilitation. Viewed from another angle we can say that the person was suggestible. On the other hand, the set of the individual might be such that it opposed the sort of reaction that the outside situation demands. Here we have a competitive situation and the external stimuli might be neutralized by the internal set and the person not respond at all. In such circumstances the person would appear to be negativistic. To say a person is suggestible is only another way of saying that he is tuned to respond to some external situation; to say he is negativistic is another way of stating the fact that his internal set is opposed to the external stimuli of the moment.

Not only may the same individual differ in the relation of his attitude toward an external situation at different times but there is a fairly constant attitude that may be attributed to the same individual in different circumstances. We may have a person who is habitually suggestible or negativistic. The fact that the internal set harmonizes with and reinforces the external stimulus situation has been called dynamogenesis, but this term actually attributes potency in some mysterious manner to the internal set when as a matter of fact the apparent dynamic force is simply the result of harmony.

So far we have considered only conditions where the organism as a whole either harmonizes or antagonizes the external situation. In these cases we have what is usually considered normal suggestibility or negativism. There is another factor that needs to be considered and that is the possibility of parts of the organism acting independently. This gives rise to what are known as automatisms. The responses which involve the whole unified organism are known as expressive movements; that is, the external situation acts as a suggestion which sets off a response in harmony with the set of the individual. In automatic movements the suggestion or external stimulus is in harmony with a partial set and causes a response which may be quite foreign to

another part of the personality. The response is automatic in that it occurs without the full coöperation of the individual. In order for an automatic movement to occur there must be a certain degree of dissociation of the personality.[1]

The degree to which persons can respond to a suggestive situation or can dissociate so as to respond partially to a stimulus while partially occupied with another situation varies greatly in the same individuals at different times, and in different circumstances, as well as in different individuals. Binet[2] found that children differed in suggestibility and thought that this might be related to intelligence. Stein[3] found that in some subjects intense mental application favored instead of inhibiting automatic movements. Janet, as stated above, held that suggestibility is the criterion of hysteria; that an hysteric has to an abnormal extent yielded to the influences of certain situations until they have an overwhelming influence upon him; he cannot possibly resist the influence of the situations. Janet has also shown very clearly a direct relation between minor degrees of dissociation such as occur in automatic movements, sleep walking, fugues and the extreme dissociations of hypnosis and double personality.

Freud describes suggestible persons as those showing the possibility of transfer, by which he means those persons who are able to bestow their affection upon another, to take others into their confidence and to receive advice and suggestions from others. This type of person, if he develops a neurosis, is called by Freud a transference neurotic. By this he means one who has a neurosis which can be projected upon another individual. Such a patient is accessible, his confidence can be won and there is a possibility of curing him by analysis. Over against this type of person he describes the one who is negativistic, from whom a transfer cannot be obtained. If this type of person becomes neurotic, he becomes introverted,

[1] Ladd and Woodworth, 'Elements of Physiological Psychology,' Scribners, 1911, p. 534.

Pierre Janet, 'The Major Symptoms of Hysteria,' Macmillan, 1913.

[2] 'La suggestibilité,' Paris, 1900.

[3] PSYCHOL. REV., 1898, 5, 295.

turns in on himself, will take no one into his confidence and resists all attempts of an outsider to give him suggestions.

All these facts point to the conclusions that *suggestibility is an attitude or set on the part of the individual which may be a temporary or chronic attitude, which may involve the whole organism and thus be what we call an expressive attitude or it may involve only a part of the individual's personality and involve dissociation, and, finally, that this dissociation may develop to such an extent that one shows a double personality.*

## The Development of Suggestibility

There may or may not be innate differences in the tendency to suggestibility. There seems to be no way at present of determining this fact; but regardless of this uncertainty there are certainly environmental influences that tend to promote this attitude. We know that a child learns to do things in the way that nets him the greatest satisfaction. If, by following suggestions that are given him, he gets more satisfaction than he does by doing things in contradiction to suggestions, he learns that this attitude pays in terms of satisfaction and will no doubt tend to retain it.

It may be that the pleasure which results from the adoption of a certain attitude is not the only determining factor. Freud has posited the existence of another element which he calls the 'repetition compulsion.' He says,[1] "Thus one knows people with whom every human relationship ends in the same way: benefactors whose protégés, however different they may otherwise have been, invariably after a time desert them in ill-will, so that they are apparently condemned to drain to the dregs all the bitterness of ingratitude; men with whom every friendship ends in the friend's treachery; others who indefinitely often in their lives invest some other person with authority either in their own eyes or generally, and themselves overthrow such authority after a given time, only to replace it by a new one; lovers whose tender relationships with women each and all run through the same phases and come to the same end, and so on. We are less astonished

[1] 'Beyond the Pleasure Principle,' International Psychoanalytical Library, No. 4, 1922, London, pp. 22–24.

at this 'endless repetition of the same' if there is involved a question of active behavior on the part of the person concerned, and if we detect in his character an unalterable trait which must always manifest itself in the repetition of identical experiences. Far more striking are those cases where the person seems to be experiencing something passively, without exerting any influence of his own, and yet always meets with the same fate over and over again. One may recall, for example, the story of the woman who married three men in succession, each of whom fell ill after a short time and whom she had to nurse till their death. . . . In the light of such observations as these, drawn from the behavior during transference and from the fate of human beings, we may venture to make the assumption that there really exists in psychic life a repetition-compulsion, which goes beyond the pleasure-principle. . . . There remains enough over to justify the assumption of a repetition-compulsion, and this seems to us more primitive, more elementary, more instinctive than the pleasure-principle which is displaced by it."

Whether one uses the concept of the pleasure-principle or the repetition-compulsion to explain the repetition of complex reaction patterns or attitudes, the fact remains that one can learn to adopt a certain attitude in successive complex situations. One's attitude is an educable factor. The normal individual gets a balance between tendencies to adopt a suggestible attitude and a negativistic attitude and so shows no marked trend in either direction. Even the normal individual is inclined to react without due deliberation in situations where the emotional concomitant is strong and so even the normal person at times shows tendencies toward either attitude. Hence we can take a normal person and under different conditions cause him to show either a negativistic or a suggestible attitude. Persons who are slightly unstable will show either tendency more easily.

It is certainly more plausible to think that attitudes or emotional sets are more determined by such environmental training than by any innate tendency. Even if there does exist an innate tendency in either direction it takes a concrete

turns in on himself, will take no one into his confidence and resists all attempts of an outsider to give him suggestions.

All these facts point to the conclusions that *suggestibility is an attitude or set on the part of the individual which may be a temporary or chronic attitude, which may involve the whole organism and thus be what we call an expressive attitude or it may involve only a part of the individual's personality and involve dissociation, and, finally, that this dissociation may develop to such an extent that one shows a double personality.*

## The Development of Suggestibility

There may or may not be innate differences in the tendency to suggestibility. There seems to be no way at present of determining this fact; but regardless of this uncertainty there are certainly environmental influences that tend to promote this attitude. We know that a child learns to do things in the way that nets him the greatest satisfaction. If, by following suggestions that are given him, he gets more satisfaction than he does by doing things in contradiction to suggestions, he learns that this attitude pays in terms of satisfaction and will no doubt tend to retain it.

It may be that the pleasure which results from the adoption of a certain attitude is not the only determining factor. Freud has posited the existence of another element which he calls the 'repetition compulsion.' He says,[1] "Thus one knows people with whom every human relationship ends in the same way: benefactors whose protégés, however different they may otherwise have been, invariably after a time desert them in ill-will, so that they are apparently condemned to drain to the dregs all the bitterness of ingratitude; men with whom every friendship ends in the friend's treachery; others who indefinitely often in their lives invest some other person with authority either in their own eyes or generally, and themselves overthrow such authority after a given time, only to replace it by a new one; lovers whose tender relationships with women each and all run through the same phases and come to the same end, and so on. We are less astonished

[1] 'Beyond the Pleasure Principle,' International Psychoanalytical Library, No. 4, 1922, London, pp. 22–24.

at this 'endless repetition of the same' if there is involved a question of active behavior on the part of the person concerned, and if we detect in his character an unalterable trait which must always manifest itself in the repetition of identical experiences. Far more striking are those cases where the person seems to be experiencing something passively, without exerting any influence of his own, and yet always meets with the same fate over and over again. One may recall, for example, the story of the woman who married three men in succession, each of whom fell ill after a short time and whom she had to nurse till their death. . . . In the light of such observations as these, drawn from the behavior during transference and from the fate of human beings, we may venture to make the assumption that there really exists in psychic life a repetition-compulsion, which goes beyond the pleasure-principle. . . . There remains enough over to justify the assumption of a repetition-compulsion, and this seems to us more primitive, more elementary, more instinctive than the pleasure-principle which is displaced by it."

Whether one uses the concept of the pleasure-principle or the repetition-compulsion to explain the repetition of complex reaction patterns or attitudes, the fact remains that one can learn to adopt a certain attitude in successive complex situations. One's attitude is an educable factor. The normal individual gets a balance between tendencies to adopt a suggestible attitude and a negativistic attitude and so shows no marked trend in either direction. Even the normal individual is inclined to react without due deliberation in situations where the emotional concomitant is strong and so even the normal person at times shows tendencies toward either attitude. Hence we can take a normal person and under different conditions cause him to show either a negativistic or a suggestible attitude. Persons who are slightly unstable will show either tendency more easily.

It is certainly more plausible to think that attitudes or emotional sets are more determined by such environmental training than by any innate tendency. Even if there does exist an innate tendency in either direction it takes a concrete

situation to enable it to develop and it is perfectly conceivable that a person who has a tendency in one direction may be modified by education so as to develop a normal balance or even a tendency in the opposite direction.

## The Experimental Approach

In the light of the theoretical viewpoint above outlined attempts have been made in this laboratory to determine whether the hypotheses set forth are true or false. In order to put this to a test it was necessary to discover some objective way in which the suggestibility of a person could be measured, to see whether normal persons showed either tendency markedly and to determine whether persons who were known to be either negativistic or suggestible showed a consistent reaction in the test situation.

Now it happens that we have a ready criterion of extreme suggestibility and negativism in two types of abnormal individuals. It is well known and is clearly presented in descriptions of schizophrenics that they are negativistic with a peculiar splitting in the coördination between the intellectual and emotional life. This coördination disturbance may show itself in different ways; a lack of harmony between the expression of the emotion and the idea content of the thought, an inadequate affective reaction, an affective reaction from wholly indifferent causes, a poverty of affect, or a sort of instability of affect which has been described as 'April weather' behavior. In dealing with these cases one is impressed with the feeling that the schizophrenic exists in an entirely different world from one's own. He has shut himself into an existence where one cannot get access to him. One has the suspicion (a suspicion which is not subject to proof) that the emotional expressions are real, but are to things which the patient succeeds in keeping completely from the observer. He is so constantly on his guard that one can never break through his defenses. He will laugh, but he will not laugh at our suggestions. He will cry, but not when we present a situation which suggests crying. Suggestions simply do not register. In such a case we have the supreme development of a negativistic attitude.

The question arises as to whether there is a direct developmental relation between the simple negativistic attitude of a child who has been taught that he gets along better when he opposes suggestions and this person who seems to be thoroughly convinced that outside suggestions are not to be responded to. The possibility of such a developmental relationship forms an interesting and valuable hypothesis and it was this hypothesis that suggested in part the development of the experimental procedure which led to the method we are about to describe of distinguishing the suggestible from the negativistic.

Over against the schizophrenic individual is another type of abnormal person who has just the opposite characteristics. This is the psychoneurotic type who is described by Ferenczi as either loving or hating the whole world. These individuals when they have reached an extreme stage can become completely subdivided into separate personalities, as has been amply shown by Janet, Prince and others. Janet has shown that the characteristic feature of these cases is the tendency to dissociate, either partially or wholly. This dissociation enables the patient to give attention and react to two or more definite situations at the same time. To be busy with one task and react to an outside stimulus at the same time surely means that one is not very much on his guard against outside stimuli. He certainly must feel that it is perfectly agreeable with his personal ego to react to outside situations or he would not carelessly do so with his conscious attention on some other ideas. Hence here we have the direct antithesis of the schizophrenic type. The latter is on his guard against stimuli from the outside, the psychoneurotic has the bars down and will react to such suggestions without the least conscious control. The normal person lies somewhere between these two extremes and at times in certain situations reacts easily to suggestion and at other times becomes negativistic.

Hence, in these two abnormal types we have a definite criterion with which to correlate the results of our test which we designed to measure this trait. If all psychoneurotics

show one thing and all schizophrenics show the opposite we have good evidence that our test is measuring the trait which so clearly distinguishes the two.

Even the person who is abnormally suggestible or negativistic does not show these traits in situations where he has complete control; where they do appear is when the individual, normal or abnormal, feels that he lacks complete control. In such a circumstance of partial or complete helplessness the suggestible person simply relaxes and gives himself up to the situation, trusting that, as in the past, things will go well. The negativistic person, on the other hand, feeling that things are slipping, is filled with a fear which is the product of previous failures. He immediately places himself on guard and resists all suggestions for fear he may think thoughts or do acts which are contrary to his ego. The suggestible person does and thinks things and either repents afterwards or forgets completely that he did or thought them. The negativistic person repents before he slips, or tries to ignore the fact that the suggestion presented itself.

Now the experimental or laboratory situation which most nearly approaches this condition of losing one's grasp is that of daydreaming or reverie, and we have found that these two types of person react very differently to instructions to abandon themselves to reverie. The suggestible person delights in reverie, he abandons himself to all sorts of visual situations some of which are in keeping with his ego ideals and some of which may not be. He feels no compunction for such daydreams and after coming out of a period of such abandon laughs at the situations he pictured. He feels no moral responsibility for them at all. The negativistic person has an entirely different attitude. When instructed to abandon himself to reverie he will object—he never did give control of his thought processes and he does not propose to begin now—what if some evil thought should come—such a procedure is absurd, etc. If coaxed he will finally say, "Well, I will try," or will get more negativistic and absolutely refuse to coöperate. You either get halfhearted coöperation or none. Such a person is very suspicious of the experimenter's

motives. He is afraid something will be learned about him. The suggestible person usually does not concern himself about the motives behind the experiment and is not concerned what is learned about him. One negativistic person in the midst of an experiment was not responding properly; and, in order to see what was wrong, the experimenter came suddenly into the experimental room. Here he found the subject busily taking apart the apparatus in order to discover what tricks were being played upon her. Such suspicion would never occur to the suggestible person.

There are several facts that were considered in adopting a definite experimental procedure. First, it is known that when the entire organism is intact a reflex is not so pronounced as when that reflex is separated from the cortical control of the person. The knee jerk, for example, is exaggerated when the spinal cord is severed. In other words, the cortical connections exercise an inhibitory effect on the functions of the reflex. This state of partial readiness makes the reflex available for more complex functions, but reduces its automatic response to the single reflex stimulus.

In the same way there is evidence that a sensory organ is less sensitive when the entire organism is intact and is reacting to stimuli from all fields than when other fields are blocked off and the individual is free to respond to that field alone. If one is to exercise the keenest vision he must be in quiet surroundings and free from tactile and other sensory irritations. This is shown in hypnosis, for example, where one's attention can be fixed to an abnormal extent on the sensations from one sense organ. In such a case the capacity of that organ becomes more acute than in waking life.

In general terms we may state the above facts as follows: When the organism is on its guard (in a state of readiness) to perceive any stimulus and to act in any way that the situation may demand, the sensory acuity of any receptor and the readiness of any effector is diminished in proportion to the degree of spread of this readiness to perceive and respond.

Viewing our hypothesis in the light of this generalization it is evident that an attitude of negativism means a wide-

spread guarding or readiness to respond to any contingency. Suggestibility means a lack of guard, or lack of readiness to respond to any possible contingency with an increased efficiency for reaction to particular narrow situations. It is this that is back of automatisms and double personality. If we can show that a person when placed in a situation which endangers his control of the situation becomes less acute in his sense perceptions than when in a situation over which he feels that he has complete control, we are pretty certain that this individual becomes guarded in such a situation. His fear of loss of control is manifest in the decreased acuity in any one sphere. In other words, if a person shows a difference in perceptual limen in two sets of circumstances, we can infer that the decrease is due to inhibiting factors and these inhibiting factors are synonymous with the spread of attention to other stimuli than the one in question.

In line with these facts the situation into which we placed the subject to measure these traits was one of abandon or reverie. In order to facilitate this abandon we used a dark-room and had the subject look into an illuminated crystal. These accessories are not absolutely essential, however, for we have secured the same results with a room that was by no means sound-proof, that was not thoroughly dark, and with subjects who did not look into the crystal. Any circumstance that facilitates reverie is desirable, but the success of reverie depends more upon the attitude of the subject than on experimental conditions. We found it very important, for example, to make it very clear to our subjects just what we meant by reverie. The suggestible ones usually needed very little instruction but the negativistic ones and an occasional suggestible one would confuse reverie (or day-dreaming) with concentration upon a problem. They did not seem to be able to grasp the notion of abandon.

Briefly, our experimental procedure was as follows: Each subject was given a test to determine the lower intensity limen for sound. He was then told to abandon himself to reverie or daydreaming and while in this state of abandon sounds were given again through a receiver clamped to his

head. He was instructed to make no effort to hear sounds through the receiver but when they came to respond to them by pressing on a key. After taking the threshold under these conditions he was told to pay strict attention to the sounds, to cease daydreaming and his threshold was again taken.

This procedure was used with cutaneous stimuli with results similar to those for auditory stimuli showing that the results are due to a central set and not to any peripheral factor.

## Conclusions

1. Some persons show increased sensory acuity during daydreaming while others show decreased acuity under the same conditions. This change in sensory acuity is usually very clearcut.

2. Among the normal individuals it was found in a general way by observation that those whose thresholds went down during reverie were of the suggestible type and those whose thresholds went up were of the negativistic type.

3. Almost invariably psychoneurotic patients had lowered thresholds during reverie and schizophrenic patients had raised thresholds.

4. During hypnosis the threshold of the subject is lowered and the degree to which it is lowered is proportional to the depth of hypnosis.

5. Every person whose threshold lowered during reverie and upon whom the attempt was made could be hypnotized and not a single one whose threshold raised could be hypnotized.

6. In persons whose threshold goes down markedly there is often amnesia for reactions during the period of reverie, showing that the response is at times of the nature of an automatism.

For these reasons it is believed that we have here a very reliable way of determining the attitude of any individual toward life in general. If he is suspicious, wary, negativistic and tends toward schizophrenia his threshold will go up when told to daydream. If he is trustful, open, suggestible, and tends toward psychoneurosis his threshold will go down during

daydreaming. If he is neither one nor the other it will be practically the same under all conditions.

Since this test was devised it has been used on a large number of cases in the Psychopathic Hospital and has merited the confidence of all who have to do with these patients. It serves as a laboratory check on the clinical diagnosis when there is a question between schizophrenia and psychoneurosis.

The value of this test should be very far-reaching. It is based on the theory that a psychoneurosis or a schizophrenic psychosis is the product of a functional development; that is, either condition is a learned attitude which is acquired just as any habit is learned and is governed by the laws of habit formation. If this is so one can use this test with children or young persons and can discover whether either tendency has already developed and counteract such a tendency by training in the opposite direction, thus producing a normal balance. Furthermore, when a patient is first brought for examination the use of this test is valuable in the sense that it gives an almost immediate index as to which type of person is before the examiner.

While a test formulated on the basis of a functional theory of the neuroses does not indicate the truth of the functional background even when the test has proved to be successful, it at least furnishes a working basis in the cure of these cases until some one can definitely prove that the theoretical background is false. There have been enough cases of cure of the psychoneuroses by a change in the environmental situation to lend weight to the psychogenetic background of this type of mental disturbance. There have been so few cures of a well-developed schizophrenic psychosis that such cases furnish little evidence in either direction. However, since the use of this test may serve to indicate a person who has a schizophrenic trend while he is still in the early stages, we have the hopes of doing valuable work with these cases in the direction of reëducation. The reason for the previous failures with the schizophrenics does not indicate that this disease has any more of an organic background than does a psychoneurosis,

but simply that in the very nature of the case as one develops in the direction of schizophrenia he becomes more negativistic, more inaccessible and hence less amenable to any educational reconstruction. The real value of the test is then not in its use on well-developed cases of mental aberration but in the testing of normals to avert any possible breakdown.

The real fight against any disease does not come until we have a laboratory test whereby the first stages of the disease may be discovered and its progress combated. Prevention involves early discovery with immediate application of a suitable preventative. This test gives us a laboratory method of discovering a developmental tendency before it is well established. The negativistic individual can be changed by a proper application of true pedagogical methods before he becomes inaccessible and a permanent cure effected before the appearance of a condition that even looks like an actual disease. One does not combat a diphtheria epidemic (for example) by waiting until the disease shows itself clinically; he makes a laboratory test upon all suspects and nips the disease in the bud. It is believed that in this test we have a laboratory test which can be used to detect incipient tendencies toward functional neuroses and hence that it may become a very valuable instrument in mental prophylaxis and hygiene.

# THE SYMBOLIC PROCESS

BY WALTER S. HUNTER

*University of Kansas*

In a previous paper [1] upon the nature of 'consciousness,' I have attempted to substitute a matter-of-fact formulation of stimulus-response relationships for the customarily accepted indefinable psychic aspect of human nature. I have attempted to show that 'consciousness' is a term applied by the subjectivist to a phenomenon whose typical form is that of a language response aroused by sensory stimulation. Since language responses, which for brevity's sake I have designated by the hieroglyphic *LR*, are a certain kind of muscular contraction under the control of the nervous system, they stand in a bipolar relationship to sensory processes (designated by the sign *SP*). Antecedent to the muscular response is a sensory process and, due to the stimulation of receptors by these muscular activities, other sensory processes are consequent to them. This bipolar relationship can be written *SP–LR–SP*. The one specific relationship, or phenomenon, *SP–LR*, I have sought to identify with the phenomenon termed 'consciousness.' In order to have a minimum of misunderstanding, one instance of behavior has been taken as the standard to which the definition of 'consciousness' is to apply. This instance is called the Type-*X* situation, and its definition is quoted on page 485 of the present article. The *SP–LR* relationship, which formulates the Type-*X* situation in stimulus-response terms, is an irreversible one-way relationship—not irreversible in the sense that the direction of the nervous impulse is irreversible from receptor to effector, but irreversible in the sense that the *SP* concerned in the Type-*X* situation is antecedent and not consequent to the *LR*. With this brief summary of our analysis to date, let us proceed with the task of the present paper.

[1] Hunter, Walter S., 'The Problem of Consciousness,' PSYCHOL. REV., 1924, 31, 1–31.

One of the important problems for analysis which remains over from our preceding article is that of the nature of the language response. In that discussion it was pointed out that two of the most important characteristics of language responses were their symbolic character and their revivability by the organism. Of these two characteristics, the former will chiefly concern us because it contains the clue to the difference between a language and a non-language response. The particular aspect of the symbolic process which is most engaging is that of its relationship to the development of a substitute process from an initial stage until complete automatization is reached or approximated. On pages 24 and 25 of the earlier paper, in dealing with the life-history of the Type-*X* situation, it was said that the *SP–LR* relationship might become so automatic that, while the response might appear to be one of language, it would in reality be no more a true language response than would the knee-jerk following upon patellar stimulation. In a way which we have not yet presented, increasing practice serves to destroy the symbolic character of the response. And as this destruction proceeds, the *LR* undergoes a transformation which obliterates it.

A symbolic process is a substitute process of a certain kind and of a certain stage of development. It will be well therefore in developing our subject to restate briefly the general character of substitution. This we shall do in terms of the conditioned reflex. Certain stimuli will without previous training of the organism elicit certain responses. Thus an increase in the intensity of light stimulating the normal eye will produce a contraction of the pupil of the eye. Chemical stimulation of the tongue will produce a secretion from the salivary glands, and a pricking stimulation of the finger is followed by a withdrawal of the finger. Starting with this relatively simple basis, training may result in the addition of new stimuli or in the addition of new responses. Probably experiment could show that neither type of change is present without the other. Woodworth [1] has said: "The substitute response machinery is more complicated than that

[1] Woodworth, R. S., 'Psychology,' 1921, p. 408.

of the substitute stimulus. . . . Evidently because there is something wrong with the original response . . . ." That this is not necessarily the case, our above statement implies and the following case illustrates. An auditory stimulus, such as is found in the ringing of a bell, produces a dilation of the pupil of the eye. And yet if the bell is sounded while the pupil is contracting because of an increase in light intensity, the bell soon becomes an effective stimulus for pupillary contraction. Figure 1 gives a diagrammatic repre-

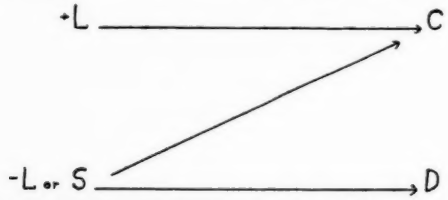


FIG. 1. A diagrammatic representation of the conditioned pupillary response. + *L*, increase of light intensity; − *L*, decrease of light intensity; *S*, sound; *D*, dilation; *C*, contraction.

sentation of the case. The change in the stimulus-response situation must be described both as a modification of the stimulus and as a modification of the response. The sound has become a substitute for the change in light intensity, and the contraction has become a substitute for dilation of the pupil. It is theoretically possible that proper experimental conditions might condition an increase in light intensity to a dilation of the pupil.

Inasmuch as a substitute always arises as a result of practice, it may be defined as any stimulus-response relationship, *i.e.* connection, resulting from training. (Standard case: the conditioned protective reflex.) One may, however, wish to emphasize a part only of the process, in which case one may speak either of the stimulus substitute or of the response substitute. It is important to point out two particular characteristics of the substitute process:

(1) It may be established without the 'awareness' of the subject (as is shown in Cason's [1] study of the conditioned

[1] Cason, Hulsey, 'The Conditioned Pupillary Reaction,' J. EXPER. PSYCHOL., 1922, 5, 108–47.

pupillary response) and in that sense therefore without 'consciousness.' Stated in our terminology, this would mean that neither the elements nor the totality of the substitute process need afford a first term for an irreversible *SP–LR* relationship.

(2) After a substitute stimulus or a substitute response has been set up, further training under the original conditions fails to change the qualitative nature of the process. Thus if sound has been substituted for injury as the stimulus for the protective reflex, additional training still leaves the sound a substitute. It is therefore to some added characteristic of the substitute process that we must look for the essential nature of the symbol so far as that is related to training, because in some way, we have said, training serves to eliminate the symbolic character of a stimulus-response connection.

Let us first inquire into the changes which occur in a stimulus-response situation with continued training. These may be listed as follows:

(1) The establishment of sensory and motor substitutes as instanced in the conditioned reflex process. In so far as the change is on the sensory side, it may be described as a shift in the sensory control of the response.

(2) An increased accuracy of response when judged by the standard of the final level of attainment.

(3) A decrease in the fatigue accompanying the execution of the response.

(4) An elimination of stimulus-response connections (random movements) which are at first involved but which are not manifest at the final level of attainment.

(5) A decrease in the time required for the completion of the responses set up by the stimulus and terminated by the experimentally chosen goal.

(6) A decrease in the time variations from one repetition to other adjacent ones, *i.e.* an increased automatization of the stimulus-response connections.

(7) A partial or total elimination of 'consciousness' of the stimulus and the response.

(8) The elimination of the symbolic character of the substitute.

If we consider such instances of behavior as the conditioned reflex and the maze-performances of the earthworm and the rat, we notice that the first six characteristics may be present without any scientifically necessary implication of characters 7 and 8. In other words, substitutes may be acquired as well as maintained without involving 'consciousness.' It is generally admitted by psychologists that a substitute process, once it has reached a high level of efficiency, may function without involving 'consciousness' and without coming within the range of a possible report by the subject. We must therefore look outside of the phenomena covered by the first six items above if we are to clarify the problem presented by the relation of items 7 and 8 to the learning process and thereby arrive at a more adequate understanding of the irreversible *SP–LR* relationship. We have already identified 'consciousness' with the symbolic character of the substitute as formulated in the irreversible *SP–LR* relationship. It now behooves us to indicate clearly how it is possible for a substitute to lose its symbolic character while still remaining a substitute.

The analysis of this phenomenon may be put most briefly in diagrammatic form. Let us first consider the case where the visual stimulation by a box leads to the verbal language response, *box*, under the influence of the instruction-stimulus "What is it?" We shall assume that the pathway (Fig. 2)

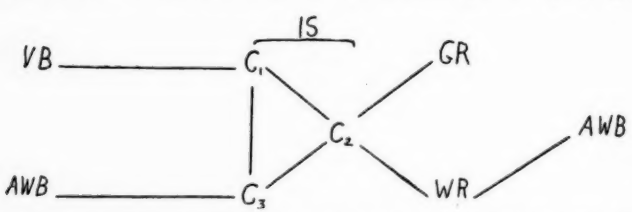


FIG. 2. Diagrammatic representation of substitutes and symbols. *VB*, visual box; *AWB*, auditory word box; *IS*, instruction-stimulus; *GR*, grasping response; *WR*, word response; $C_1$ $C_2$ $C_3$, neural centers.

*VB–C*$_1$*–C*$_2$*–GR* has already been functionally established in the individual, so that at the time when we consider him, the visual box will arouse the grasping response. We shall

also assume that the pathway *AWB—C₃–C₂—WR* has been functionally established so that the auditory stimulus of the word *box* will arouse the verbal response, *box*. We shall further assume the controlling, or limiting, influence of the observational setting or of the instruction-stimulus, "What is it?" By the conditioned reflex method, these two stimulus-response systems may be interconnected as indicated in the figure, where *IS* stands for the instruction-stimulus. Now when the visual box is presented, the subject responds verbally and may in addition reach for the box. The nervous activity diffuses through all three centers, $C_1$ $C_2$ $C_3$. The irreversible *SP–LR* relationship constituted by *VB–WR* is the phenomenon termed 'consciousness,' and the subjectivist says that the individual is conscious, or aware, of the box. Again, by a different arrangement of the training conditions, the auditory word, *box*, may set up the grasping response, and the nervous impulses may again diffuse through the centers, $C_1$ $C_2$ $C_3$. Here also we draw the same conclusion as above, viz., that the irreversible relationship, *AWB–GR*, constitutes an instance of 'consciousness' which the subjectivist would say involves a 'consciousness' of the sound. Attention should also be drawn to the fact that many centers (or forms of nervous activity) other than $C_1$ $C_2$ $C_3$ will be involved in each case which we have described. These other centers will represent the influence of training and hereditary conditions which for simplicity's sake do not enter into the figure.

The situation as it now stands in our description persists throughout a certain undetermined middle interval of the training period. During this interval, *AWB–GR* is not only a substitute for *VB–WR*, but it functions as a symbol of it. (Because of convenience and ease of observation, *GR* alone is often said to be the symbol.) And under the other conditions described, *VB–WR* (or simply *WR*) is the symbolic substitute for *AWB–GR*. With continued training, the complexity of the central connections is decreased until the pathway involved is a more direct one from sensory excitation to response. The activity is now quite automatic, and while it is still a substitute process, it is in no sense a symbol,

inasmuch as its relationship to the associated neural centers of the original process has been eliminated. Furthermore, observation reveals that at this stage of the training that which the subjectivist has termed 'consciousness' has likewise disappeared. It is also at this time that the subject becomes unable to report "I saw the box" or "I heard the sounds." Now since the decrease in central connections is the chief change which has occurred (as evidenced by the loss of random movements and the increased automaticity of the response), it seems very probable that the verbal report is essentially conditioned by the symbolic character of the stimulus-response relationship.

Attention should now be invited to the very important fact that each of the responses with which we have dealt may be aroused by the organism, *i.e.* each is under the organism's control through the associative mechanism. Both *GR* and *WR* become conditioned to stimuli other than those indicated in our figure and may become rearoused through such processes quite independently of stimuli which cannot be produced by the organism.[1] In the case of *WR*, it is to be noted that the response itself gives rise to an auditory stimulus essentially identical with the one (*AWB*) which was initially effective. Whenever *WR* is rearoused by neural processes containing as an integral part processes represented by $C_1$ (those conditioned to *VB–GR*), it is again a symbolic response and so constitutes a part of an irreversible *SP–LR* relationship.

This last statement leads us to the definite formulation of a question for which the statement itself is the answer. After a conditioning process has become automatic, may it ever revert to its condition in the earlier symbolic stage? The problem is an important one for the organism. We have already indicated that in the language responses the individual has at his command an adaptive process of unique value. This behavior, through its symbolic character and its initiation by the organism's own activity, makes possible an adjustment to stimulating conditions which are no longer present. The greater complexity, variability, and range of

[1] Hunter, Walter S., 'General Psychology,' 1923, pp. 288 and 299.

adaptive value of an *LR*, when compared with typical learned or unlearned behavior, is clearly evidenced in any comparison of the behavior possibilities of man and the animals below him. Almost all that Bergson claims for intelligence as opposed to instinct might be applied to the capacity for the indirection of responses found in an *LR*. If any case of a symbolic process existed only during a certain undetermined period of the training interval and if after that period it were forever merged with merely learned behavior, then the organism would be limited in its exercise of that function to those conditioned responses which were as yet incompletely established. Something fundamentally like this does occur, and still a wide range of *LR*'s remains by virtue of the exceedingly large number of uncompleted automatizations which one finds in man.

We may formulate the above account more concretely as follows: Where the stimulation by 660 $\mu\mu$ light leads directly to the verbal response 'Red,' the Type-*X* situation [1] does not exist. Nor does it exist when the self-produced auditory stimulation 'Go' leads directly to the response of running. In the latter case, the stimulus is under the control of the organism through the associative machinery and is in addition a substitute for the original stimulus for the response. However, by definition, the 'Go' leads to the response as simply, as directly, and *with as little evident involvement with other stimulus-response connections* as though its genetic history did not prove it to be a substitute for a more original stimulus. *A substitute can only be identified through its past history.* No amount of immediate observation can reveal the original or derived status of the stimulus, 'Go.' In the same way the sensory process occasioned by the light, 660 $\mu\mu$, leads to a

[1] The following quotation from the paper on the 'Nature of Consciousness' (page 11) is offered in explanation of the Type-*X* situation: "Here, then, at the conclusion of this brief examination of language activity, we may repeat the statement first made that the presentation of a given stimulus with the instructions 'What is it?' may be followed by a language activity in an organism possessing: (1) a certain receptor capacity, (2) a certain habit history, and (3) language ability. This phenomenon is observable and verifiable and is typified in what we shall term, for convenience of future reference, the *Type-X situation, where the language response 'red' follows upon a certain visual stimulus and a certain instruction-stimulus.*"

verbal response 'Red' without direct observation being able to pass upon the substitute character of the response. In the light of our discussion in the previous paper, it is necessary to add that in any specific case it is extremely difficult to determine by any means whatsoever whether or not the substitute is also a symbol. If the process is a symbol, it will still involve active associative relations with the original process. This is made theoretically probable by the stage in the training period at which the functionalist says that 'consciousness' is found and by the undoubted decrease in central connections with practise. It receives empirical support from the report of the subject. This testimony consists essentially in an effector activity which appears to be conditioned by the associative connections in question. A SYMBOL CAN ONLY BE POSITIVELY AND SPECIFICALLY IDENTIFIED WHERE RESPONSES CAN BE CONDITIONED TO THE ASSOCIATIVE TRACES OF THE ORIGINALLY EFFECTIVE STIMULUS. To say that 'the stimulation by 660 $\mu\mu$ light leads *directly* to the verbal response' is equivalent to saying that no central processes (associative traces) are present which in themselves might condition another report by the subject. In human subjects the responses "I am conscious of this," "I saw that," etc., have been conditioned to these associative traces of stimulus-response connections. In the animals below man no such evidence of specific symbolic processes is available. Here one can only depend upon such general evidence as the delayed reaction experiment may offer.

For those of us who are aided by diagrammatic presentations, the analysis of Fig. 3 will be helpful. This figure purports to indicate the receptor-effector relationships in the subject who has passed through training periods prior to this particular one. The visual stimulation by the box, $VB$, sets up impulses directly through $C_1$ and $C_2$ to $WR_1$. Although substitute processes are present, no symbolization is involved, and no associative connections are present which permit a report, $WR_2$, by the subject. If, on the other hand, $VB$ sets up a neural excitement which involves $C_1C_2C_3C_4C_5C_6C_9$ and

terminates in $WR_1$, then the substitute process is also a symbolic process. The associative traces other than $C_1C_2$ may at a later date condition $WR_2$, the subject's report. In other terms, the two following irreversible *SP–LR* relationships are present: (1) $VB—C_1–C_6C_9—WR_1$, and (2) $Y—C_1–C_6C_9—WR_2$. Each of these would be an example of the Type-*X* situation. (*Y*, in the formula, stands for the associative cue which rearouses the central process and which is not indicated on the diagram.)

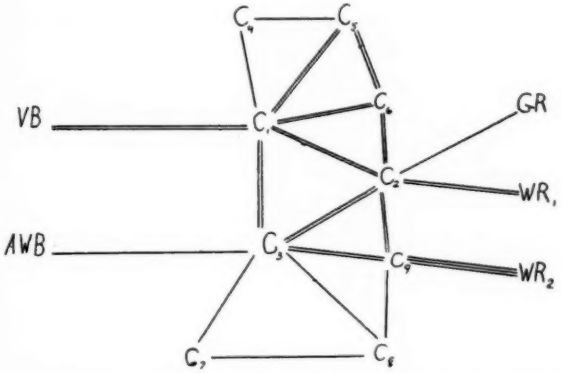


FIG. 3. The connections represented in double lines indicate the general nature of the neural basis of an irreversible *SP–LR* relationship as discussed in the text. $WR_2$ is an *LR* made possible by the complex central connections which have not yet been eliminated, *i.e.* reduced to a $C_1$ $C_2$ basis.

Although the diagrams of Figs. 2 and 3 are theoretical constructions and are explanatory and not descriptive of behavior, the following points can be made in their support: (*a*) Behavior is controlled by neural processes working through a complex of centers. (*b*) There is an elimination of certain neural paths as the final integration of stimulus and response is approximated. And (*c*) after the integrative process has attained a certain degree of perfection, it no longer conditions a report by the subject.

Let us turn our account briefly in the direction of description. Suppose we set our subject the task of learning to say: "The — curfew — tolls — the — knell — of — parting — day."

Training may be conducted to such a point that, when the subject is stimulated by "Did you say, 'tolls the knell,' " he may be unable to report, "I did." The simplest explanation is that associative traces making possible a conditioning of the response "I did" were inactive in arousing the response "tolls the knell." "Tolls the knell," therefore, symbolized nothing, was an 'unconscious' response, was not a part of an irreversible *SP–LR* relationship. It is said that the response took place automatically. Perhaps it is also said that the response was *inattentively* made. This distinction between an attentively and an inattentively performed response is the distinction between a response which can be reported and one which cannot be. More and more as training proceeds, the inattentive performance preponderates over the attentive. The subjectivist's phenomenon of 'attention' is present only when the symbolic process is active. And the readiness with which the process determines the report is probably his 'degree of attention.' We have spoken only of the later stages of the memorizing of the verse from Gray's Elegy. We might, however, have used any stage. In the earlier periods, verbal report by the subject could have been elicited more readily and frequently. This situation can be best described, as we have frequently done before, in terms of the loss of symbolic character of the stimulus-response situation, although the subjectivist would be tempted to describe it in terms of decreasing consciousness and attention.

We may then restate the conclusion reached in our previous paper with a qualification as follows: A language response is equivalent to a substitute process which can be reinstated by the organism, but ONLY where associative traces of the original process persist in the integration. Where this *LR* occurs, the organism is said to *use* the substitute as a symbol. Evidence of the genuine language character of the response can be secured (with certainty) through the accurate conditioning of further substitute responses by the associative traces and, for most practical purposes, through the utilization of the observer's clinical picture of how the organism

behaves when it is using the substitute as a symbol. It is at this point that the greatest need for experimental work exists. Most of us feel that it is possible to distinguish between responses set up automatically in an infant and responses used by the infant to symbolize certain wants. Experiment, rather than theory, should make precise the basis for this distinction.

Up to this point, the present paper has sought to point out the probable nature of the change which occurs in the neural conditions of a symbolic process in order to reduce it to the merely substitute level. We shall continue to clarify the nature of the symbolic process by working through the topics of thinking and purposefulness; and first, the question of thinking.

We can perhaps make the significance of our position clearer by contrasting with it certain views presented by Watson in his book 'Psychology from the Standpoint of a Behaviorist' and in his article 'Is Thinking Merely the Action of Language Mechanisms?' [1] First a series of quotations from the above sources:

> It is well to draw distinction between a vocal habit and a language habit. By vocal habit is meant the mere sounding of words of the non-instinctive type. The word has to be learned, but it may be learned as the parrot learns it. It has not yet been connected up with other verbal action and with general bodily actions. (Book, p. 318.)
>
> Vocal acts or habits, however numerous they may be, do not become language habits until they become associated with arm, hand and leg activities and substitutable for them. (P. 319.)
>
> If our view is correct, it [thought] is a constituent part of every adjustment process. It is not different in essence from tennis-playing, swimming or any other overt activity except that it is hidden from ordinary observation and is more complex and at the same time more abbreviated so far as its parts are concerned than even the bravest of us could dream of. (P. 325.)
>
> The question is often asked what marks off thinking from the mere subvocal unwinding of well-organized language habits. (Article, p. 89.)
>
> In our opinion, 3 [solution of new and important problems] represents a bit of behavior on the part of the human animal which, when stripped of its non-essentials, is exactly like that bit of behavior which the rat exhibits when put into a complicated maze for the first time. When it gets to the food the autonomic strivings die down and it goes to sleep. . . . Surely a similar thing takes place in man. He works verbally . . . until certain verbal acts . . . are executed. If, when this conclusion is reached, the driving stimuli (verbal, autonomic, emotional, etc.) cease to operate, the adjustment has been completed. (Article, p. 91.)

[1] *British J. Psychol.*, 1920, 11, Pt. 1, 87–104.

> The behaviorist believes that thinking in the narrow sense where new adjustments are made corresponds to the trial-and-error process in manual learning. The process as a whole consists in the organized interplay of laryngeal and related muscular activity used in word responses and substitutive word responses; . . . . (Article, p. 98.)
>
> Thinking, in the narrow sense where learning is involved, is a trial-and-error process *wholly* similar to manual trial and error. (Italics mine. Article, p. 104.)

The above quotations are intended to bring out the following points: (1) Watson distinguishes between true language and a merely vocal habit. (2) He assumes that a response which is once a language habit is always a language habit. And (3) he fails to utilize the distinction between true language and merely vocal habit after it is once made, even in the case of thought.

We are moved to make the following observations upon Watson's position as an aid to the better understanding of the irreversible *SP–LR* relationship. If it is necessary to distinguish between a vocal habit and a language habit, the separation can only be one which applies to language responses *vs.* a large number of other forms of behavior of which vocal habits are but a specific case. In this event language responses will differ significantly from tennis-playing, golf-playing, and the behavior of a rat running a maze. Thinking *is* different 'in essence' from these forms of activity in the opinion of most psychologists. In our opinion this difference lies fundamentally in the fact that thinking involves a certain amount of language activity. (A careful reading of Watson's contribution to the Symposium leads to the conviction that language is not there regarded as an essential part of thought.) Watson's account of thinking is acceptable, therefore, only if he admits a significant difference between thinking and the running of the rat through the maze. No one has ever suspected the rat of having any kind or degree of language capacity, and yet it is notorious that he can be the possessor of substitutes in his behavior equipment. Language responses may be built up in the same general manner as non-language habits and may be constituted by muscular contractions as is also the case with non-language behavior, and yet it does not follow in any sense that there are no significant

differences between the two classes of response. These differences have not as yet been clearly formulated upon an experimental foundation. The existence of the differences, however, is well recognized, and the formulation of the irreversible *SP–LR* relationship seeks to clarify them.

If thinking is to be recognized as a distinguishable phenomenon of human nature, the simplest and most abbreviated type of thought is to be found in language behavior and is therefore indistinguishable from the irreversible *SP–LR* relationship itself. In the typical form, however, in which thinking is usually identified, the process possesses the following characteristics: (*a*) it is a sequence of distinguishable processes; (*b*) the sequence follows upon the formulation of a problem *by* the individual organism; (*c*) this formulated problem shapes the nature of the ensuing processes until the problem is abandoned or solved; (*d*) a significant number of the elements of the sequence can be aroused through the self-stimulation of receptors; (*e*) irreversible *SP–LR* processes occur; and (*f*) the train of events comprising this particular act of thought is terminated when the sequence of behavior ceases to concern the problem formulated. If we examine the above description of the thought-process with the intention of comparing it with such activities as (1) the formation of a maze-habit by rats and (2) the execution of perfected typewriting responses in man, the following likenesses and differences are apparent:

Thinking is like maze-learning, in that:

(*a*) it involves an acquisition of substitutes;
(*b*) it involves muscular and glandular responses to present stimuli;
(*c*) it is a process of adjustment;
(*d*) it is influenced by a 'drive' and ceases when the 'drive' no longer controls the train of responses;
(*e*) it is partially determined by the setting in which the response occurs; and
(*f*) it involves control through the self-stimulation of receptors.

The following are necessary characteristics of thinking but not of the other two types of behavior:

(*a*) a formulation of the problem by the organism;
(*b*) the irreversible *SP–LR* relationship; and
(*c*) the use and acquisition of non-automatic connections of the symbolic type.

Thinking is like using the typewriter in all of the above respects except that typewriting is more a use than an acquisition of substitutes.

The conclusion is forced upon us, then, that thought differs fundamentally from the general formation and use of habits in that it involves the exercise of language responses. The problem for solution is formulated by these symbolic processes aroused by the organism's own activity. Although the individual may have the problem set for him by another, yet unless it is incorporated into his behavior-system through symbolic representation, the ensuing process is not thought. This may be illustrated as follows: The knee-jerk in response to adequate patellar stimulation and the pupillary reflex to increased light intensity are each methods of meeting problematic situations. Each serves to adapt the individual to his environment. Each may therefore be termed purposeful. There is, however, no experimental evidence that either is ever controlled by the purpose, or problem, as formulated by the individual in whom the behavior occurs. It is possible to learn the nature of the purpose served by these responses, but the resulting *SP–LR* process does not then control the behavior. In the case of the wink-reflex, on the other hand, not only can the above described procedure be accomplished, but an irreversible *SP–LR* relationship can be aroused, through the associative machinery, which may then control or condition the wink-response. In this case not only is it said that a problem is met, or a purpose accomplished, but it is said that the individual winks with a 'conscious' purpose. Purposeful behavior is behavior controlled by a problem formulated by the individual. And 'formulation of a problem by the individual' involves an *SP–LR* relationship similar to that which occurs when the experimenter gives his subject certain instructions. It is in this sense that thought is said to be 'voluntary' and 'conscious.' Behavior determined at critical points by the associative mechanism and under the control of a formulated problem to be solved *is* thought

(*is* purposeful and voluntary behavior, if one wishes to use these terms).

Our account here turns inevitably to a consideration of the nature of purpose, inasmuch as we have found purpose to be an integral part of thought and inasmuch as thought has been found inseparably connected with the symbolic processes which are the vital characteristics of the irreversible *SP–LR* relationship. As in the case with thought, we may begin our discussion of purpose with a quotation. This we select from Warren's 'Human Psychology' [1] as follows:

"The fact that we are able to picture future situations is deemed by some psychologists to constitute a distinguishing mark of conscious behavior. This conscious component is supposed to differentiate intelligent from non-intelligent acts. Purposive thought, like other types of thought, leads to motor activity; and if the ability to picture the future is solely a character of consciousness, it follows that consciousness is itself a factor in determining the course of activity. In other words, according to this view consciousness is not merely a *characteristic* of neural activity; but it is a *force* or guiding activity in itself. This interpretation of purposive activity is called *Voluntarism*, since it emphasizes in peculiar fashion the volitional type of experience."

There are important points of similarity between our account previously sketched and Warren's statement of voluntarism, in spite of the fact that, inasmuch as voluntarism is not a proper characterization of our position, we do not make of 'consciousness' a separate aspect of the universe. Our position in agreement with the one described by Warren would make, in the first place, the formulation of instructions or problems a result achieved solely by the irreversible *SP–LR* relationship. In the second place, we have continually stressed the directive value of the Type-*X* situation. The adjective 'intelligent' we should reserve for use in other connections than the present.

McDougall, who regards all behavior as purposeful, has made certain analyses of purposefulness which it will be useful

[1] Warren, H. C., 'Human Psychology,' 1919, p. 428.

to present. His most recently expressed position [1] may be stated as follows:

I. Behavior possesses the following characteristics: (*a*) a certain spontaneity, (*b*) persistence independent of the continuance of the impression which may have initiated it, (*c*) variation of direction of persistent movements, (*d*) termination when the behavior has brought about a particular kind of change in the situation, (*e*) preparation for the new situation toward the production of which the action contributes, and (*f*) some degree of improvement when it is repeated by the animal in similar circumstances.

II. "Now, when the movements of a human being exhibit the first five marks of behavior, we do not hesitate to infer that they are purposive; *by which we mean that they are made for the sake of attaining their natural end, and that this end is more or less clearly anticipated or foreseen.*" (Italics mine. P. 47.)

III. "Purposive action is, then, action that seems to be governed or directed in some degree by prevision of its effects, by prevision of that which still lies in the future, of events which have not yet happened, but which are likely to happen, and to the happening of which the action itself may contribute. *Purposiveness in this sense seems to be the essence of mental activity;* . . . ." (P. 48–9.)

We shall comment upon these three statements in the reverse order of their presentation. Number 3, with the exception of the italicized portion, is a statement which can be accepted as a description of a type of phenomenon in human nature. The term 'mental activity,' however, introduces uncertainty and great controversial possibilities without a probability of fruitful result. The influence exerted upon behavior by instructions formulated primarily or secondarily by the subject is a verifiable occurrence, but I know of no other instance in addition to this case. Such a case of behavior we have signified our willingness to designate 'thought,' but the term is of minor importance. 'Mental activity,' however, is so indelibly linked with Mind and Consciousness, both in general and by McDougall, that the italicized statement must be rejected for reasons that, whether agreed to or not, must be evident to the reader.

The second statement of McDougall's must be most unqualifiedly rejected as a contribution to the science of human nature. It involves a vicious error, viz., that of accepting as a fact of science a theoretical interpretation which goes beyond observable data and which, moreover, leads only to further over-generalization rather than to additional observa-

[1] McDougall, Wm., 'Outline of Psychology,' 1923.

tional material. It must be insisted that there is not a scintilla of evidence to show that such 'purposive behavior' exists below man. *Of course*, it *may* be there, in the sense of the italicized passage, but no scientific man can assume its presence without evidence, any more than he can assume color-vision in animals without proof. There is not the slightest necessary connection between the first five marks of behavior and the italicized passage in question. They cannot therefore constitute a proof for a purpose of which the individual is conscious. A language report is the only evidence of 'foreseen results' which an individual can give to himself or to others.

Let us now turn briefly to McDougall's characteristics of behavior. We shall at the very beginning admit that there are differences between chemical reactions (outside of the living organism), the life-processes in plants, and the behavior of animals. As a consequence one would not expect to observe the same phenomena in a chemical laboratory which he can observe in an animal. Whatever else the animal is, it is established beyond reasonable doubt that it is a chemical laboratory *all in itself*, possessing and manufacturing new chemicals which can be released into the retort upon suitable stimulation. This is sufficient to account for all of the characteristics which McDougall advances. Let us consider the normal activity of the stomach during the period when the individual eats suitable food. The biological purpose of this activity is the mixing, partial assimilation and the conveyance of the food-substances to the intestines. The glandular and muscular responses show a 'certain spontaneity' in their onset, a persistence beyond that of the stimuli which initiated them, and a variability of response. They also terminate when a certain change has been effected towards which change they have indeed contributed. There is furthermore a well-known adaptive, or learning, capacity involved, whereby the stomach's behavior becomes adjusted to new kinds of content and stimulation. However, no one can draw a scientifically valid conclusion from this to the effect that either the stomach or any other part or the entirety

of the individual need do these things for the sake of attaining a natural end which has been more or less clearly anticipated or foreseen. And yet this is exactly what McDougall does, guided by philosophical prepossessions and the belief that behavior should be explained from complex to simple rather than from simple to complex. He would be on ground even more valid if he interpreted the stomach's activity as embodying and working out the conscious purposes of a Supreme God. This latter hypothesis may well be true, but it lies outside the realm of science.

Before summarizing our discussion, it will be helpful to present a classification of behavior which will aid in clarifying the mutual relations of the processes with which we have been concerned.

MUSCULAR AND GLANDULAR RESPONSES TO STIMULATION

I. *Classified upon the basis of genetic history*
  1. Unlearned responses to stimulation
    *X. Classified upon the basis of possible membership in an irreversible SP–LR relationship*
      *A.* Those which can condition an *LR* ('Emotions' included here.)
      *B.* Those which cannot condition an *LR*
    *Y. Classified upon the basis of location of response*
      *A.* Those located in the vocal apparatus
      *B.* Those located outside of the vocal apparatus
  2. Learned responses to stimulation
    *X. Classified upon the basis of the degree of learning*
      *A.* Completely automatic, and therefore not open to report
      *B.* Incompletely automatic, open to report
    *Y. Classified upon the basis of symbolic character*
      *A.* Language responses, *LR*
        *Classified upon the basis of location of response*
          (1) Those located in the vocal apparatus
          (2) Those located outside of the vocal apparatus
      *B.* Non-language responses
        Same sub-divisions as for *LR*

II. *Classified upon the basis of a sequence of responses chiefly controlled from within the organism*
  *A.* Sequences whose trends are determined by implicit or explicit arousal of:
    Unlearned behavior
    Learned behavior, and their sub-divisions
  *B.* Sequences not significantly controlled

It need hardly be said that this classification is not intended to portray all of the varieties of behavior nor to

indicate all of the desirable groupings of responses. It is essentially concerned with types of behavior that have made their appearance in our discussion. Its chief purpose is to keep us clear of an error commonly made, viz., that of assuming that because two forms of behavior are alike in certain respects they are therefore identical, whereas very great differences may exist. Because thinking and maze-running are alike in some respects, we are not justified in concluding that the two are alike in all essential characteristics.

## Retrospect

In seeking to describe that aspect of human nature termed 'consciousness' by the subjectivist, we have held closely to the observable data. The result has been a description of a one-way, or irreversible, relationship composed of sensory process and language response (*SP–LR*). This descriptive analysis has revealed the practical equivalence of this process and that termed 'consciousness.' It has further been possible to show that the irreversible *SP–LR* relationship has the adaptive values claimed for 'consciousness' by the functionalists.

Our analysis of the nature of the language response has taken us into the theoretical field of explanation, and it has led to a conception of the symbol in terms of associative processes in the nervous system. The further consideration of the symbolic process has led us through certain related phases of the topics of purposeful behavior and thinking. In these diverse and difficult problems, the conception of the irreversible *SP–LR* process has proved a significant and clarifying hypothesis.

# THE LADD-FRANKLIN THEORY OF THE BLACK SENSATION

BY MORRIS R. NEIFELD

*Columbia University*

It is worthy of note that in half the extensive literature dealing with color vision, the subject of black has been barely mentioned. It is not so remarkable that such is the case. In only one theory, that of Hering, is black made an essential part of the theory. In all other theories the explanation of black, the non-light sensation, stands or falls independently of the explanation of the chromatic and the achromatic light sensations. Consequently, in these, very little mention is made of black. In the Hering literature, black is naturally very much discussed—mostly, it must be admitted, in a very inconsequential way.

It has sometimes been objected to the Ladd-Franklin theory that it fails to account satisfactorily for the sensation black. Fröbes in his admirable 'Lehrbuch der Experimentellen Psychologie' gives a good account of that theory, but follows Warren in believing 'that the chief difficulty of the theory is to account for the sensation of black.' It is desirable, therefore, at this time to call attention to the views of Dr. Ladd-Franklin on this subject—views that she has definitely expressed at various times, but with, perhaps, insufficient emphasis.

Black is the psychic correlate of the absorption, by objects, of all the visible light-ray frequencies. On account of the faint gray sensations caused by endogenous retinal processes—the 'self-light of the retina'— an absolute black can be obtained only after exhaustion by preëxposure to a strong light, or by contrast (the exposure of adjoining regions of the retina to a strong light).

That black is a sensation follows from the most elementary introspection. As a psychic entity black is just as real as

any of the other visual elements. To speak of black as the negation of all sensation, as does James Ward, is naïvely to assume a relation of identity between cause and effect—a pure fallacy. In the other sense-regions it is true that the absence of a stimulus results in producing no effect, but while this may be a sufficient it is not an indispensable correlation.

Like all other sensations, black is our response to a 'definite situation,' but, paradoxically enough, and unlike all other cases, the definite situation is in the case of black the complete absence of stimulation. In other words, when we look at a black object there is no light reflected back to the eye by that object, there is no activity set up in the rods and cones, and there is no message relayed to the brain by the optic nerve. It follows that the sensation of black is a definite sensation attached to a cortical situation of inactivity or rest.

Since black is thus the result of zero stimulation, it follows again that there is only one intensity of blackness. There can be but one degree of zero. Sensations of gray, *i.e.* black-white blends, are made up of this constant amount of the sensation of blackness and a variable amount of the sensation of whiteness. Change of intensity of illumination of a gray changes not only the intensity but also the quality of the sensation: the resulting gray becomes not only brighter but also whiter.[1] In a color blend, on the other hand, increased illumination changes the brightness but not (except sometimes very slightly) the chromatic *quality*.

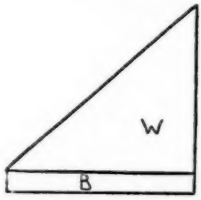


FIG. 1. Diagrammatic sketch to illustrate the range of grays. Black is of one intensity only, white is variable.

[1] The higher illumination is the cause of an increased whiteness without having any effect on the constituent blackness: the black is consequently *proportionally* less than before.

The followers of Hering make black and white one of the three ('antagonistic') disappearing color pairs. It has often enough been pointed out in the literature that this coupling of black and white destroys the very symmetry of the theory that it is aimed to produce. Complementation in the case of the other disappearing color pairs (the blue-yellows which unite to produce white and the red-greens which unite to produce yellow, produces a sensation which partakes of the nature of neither of the two members of the antagonistic pair. In the achromatic pair equilibrium results in an analyzable dual blend of the two original components of the 'antagonistic' pair. To make this plan at all workable, G. E.

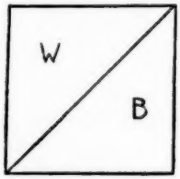


FIG. 2

Müller found it necessary, in the interest of the Hering theory to introduce the assumption of a 'constant neutral gray' sensation, due to two cortical processes which are assumed to produce, constantly, invariable and equal amounts of 'black' and of 'white.' Hering's system thus becomes tautologous.

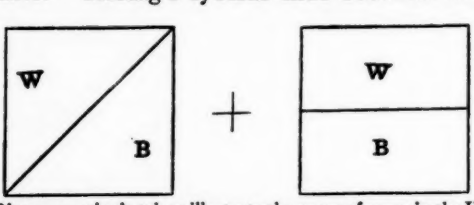


FIG. 3. Diagrammatic sketch to illustrate the range of grays in the Hering theory. Every gray consists of a constant even black-white due to cortical processes to which is added a variable black as well as a variable white due to retinal processes.

He assumes the existence of black and white, and also of a 'mean gray' which is itself a blend of black and white. But an additional assumption still is necessary, and a very improbable one, namely, that exactly in proportion as the white increases

in intensity, the black decreases in intensity. All this explaining and counter-explaining is obviated by the simple assumption (recognition of fact rather) of the Ladd-Franklin theory that the blackness sensation has for its physical correlate a zero stimulus, and that it is, consistently with this, a background sensation of one 'intensity' (or amount) only.

But the Ladd-Franklin color-theory involves also an explanation of *why we have* a sensation of blackness. The fact that black is correlated with a non-stimulated condition of the retina can be almost paralleled by the case of silence in hearing. Silence is not yet a definite sensation, but if consciousness found it necessary to attach a definite sensation to a situation of zero sound such a sensation would undoubtedly develop. We are perhaps on the way to it. We have already the phrase 'a silence that can be felt'; that is, sensed. If our entire auditory field were filled with sounds, and if we had already developed definite 'place-coefficients'[1] for sound, then in those places where sound was absent we might well have developed a 'silence sensation' to take the place of nothingness. There is no occasion for this in the case of sound, however, because our ability to localize sources of sound is very poorly developed. But in the case of vision it is different. We live every moment of our lives in a visual field. We are aware of every point of this field which is giving us a light sensation. Breaks in the field due to the absence of stimulating light, such as would occur if we had no blackness sensation, would be decidedly unpleasant. What would otherwise be 'cosmic holes' (as Dr. Ladd-Franklin has called them) in our space-field have been avoided by the fact that they are filled up with a definite non-light sensation, that of blackness. In other words when a portion of space would intrude itself into consciousness only because it aroused awareness of its vacant place, when, that is to say, the affected cortical area is at rest, then the convenient sensation of black arises.

A more primitive eye than the human would perfectly well, of course, be able to distinguish between light and the

[1] Place-coefficient is Dr. Ladd-Franklin's term to take the place of the unhappily chosen 'local signs.'

absence of light even if there were no sensation of black. An organism possessing such an eye might give the proper responses to luminescent objects without being interested in having the whole visual field filled in. In other words it follows from this that it is by no means certain that the blackness sensation arose with the beginning of vision.

The Ladd-Franklin theory, then, is far from being unable to account for all the phenomena connected with the sensation of blackness, and at the same time it permits a reasonable explanation of the puzzling question why we have any sensation of blackness at all. As black forms in sensation a simple dual color-blend with white, so this theory represents it as due to an independent physiological situation, and thus avoids the curious compensating vagaries which the Hering view necessitates.

## Summary

In the theories of color vision, the sensation of black is usually independently accounted for. In the Hering theory alone does it form an integral part of the general explanation; but the Hering view is full of inconsistencies and logical impossibilities. The only satisfactory explanation is the Ladd-Franklin view. Black is a positive sensation of constant intensity. It is the psychical correlate of a cortical condition of inactivity in correspondance to a non-stimulated retinal area. Black-white blends are the results of combinations of varying amounts of white with the one constant amount of black. Black developed as a result of nature's attempt to fill in the entire visual field in order to avoid the inconvenience of 'cosmic holes.' It is not necessarily contemporaneous in development with vision.

# SUGGESTIONS TOWARD THE REORGANIZATION OF COURSES IN PSYCHOLOGY

BY J. F. DASHIELL

*University of North Carolina*

It is an interesting fact that in psychology, in a degree far more than in any of the other natural sciences, the introductory course is still a matter of distinct interest and concern to even the leading authorities and research men of this field. The past and current rapid output of introductory texts coming in nearly all cases from accepted leaders in the subject, is sufficient attestation to this fact. No doubt this is due in large part to the fact that, in spite of the solidity and richness of its experimental data and the thoroughness with which some of its branches have been studied, it is still a battle ground of quite conflicting methods of interpretation; this due, in turn, to the fact that new modes of conceiving the subject-matter have been contributed to general psychology at different points in recent history from other fields such as psychopathology and animal psychology.

This concern over the elementary course has been mainly in regard to the types of subject-matter and the organization thereof. While some psychologists, *e.g.* hold to the primacy of sensation and the advisability of starting the course therewith, others prefer to begin with reflex action as the elemental phenomena, and still others with the presence of drives. But much less attention has been given to the somewhat more pedagogical problems of course organization for the beginning students, and in general our colleges continue to offer the introductory psychology as a one-semester or one-quarter course, the student then being turned loose on the campus and the world as one who has studied 'psychology.' Consider what he really has grasped of such phenomena as emotion or learning or instinct (with my apologies to certain psychologists for using the term) or thought-processes or motivation!

One hears the argument that a short 'overview,' 'cultural' course in the subject is a legitimate demand that should be met. The most obvious response seems to be that a so-called 'cultural' course in chemistry or physics or geology or botany is as much in order—and as little in evidence. A fair-minded consideration of the enormous mass of subject-matter falling into the field of psychology as a scientific study of human nature should leave one with a feeling of astonishment that this, probably the most complicated and the most many-sided branch in all natural science, should be thought condensable into a single short course. For that matter, is not the idea of cultural study as being a short, snappy attention to now this field and now that destined to go into the discard; and can we not more properly urge that those should enter the study of psychology who have some interest in it, and not those who are in need of a single course to fill out their program nor those who want just a nibble from each pie on the shelf? The writer recalls a committee of the American Psychological Association that recommended, as early as 1909, that the introductory course be made a year's study (on the semester plan) instead of a half year's. Some progress has been made in that direction in the fifteen years since, but surprisingly little. And for what reason? Letters to psychologists at a dozen prominent universities elicited responses indicating that the introductory course in this subject is offered as a two-quarter or two-semester course in three cases, as a single-quarter or semester course in eight cases, as both in one case. Prompted to give reasons, all those mentioning a double-quarter or double-semester course claim it to be the only adequate presentation of the subject; while all but one of those mentioning a single course explain it as due to curricular necessities, some criticizing the situation severely.

The traditional manner of handling the beginning course is quite inadequate from another and more important consideration. An introductory course is given that in nearly all cases consists almost exclusively of text-book or lecture work, and almost never anything of an experimental or a

concrete nature beyond a few perfunctory demonstrations. The student finishing this class is frequently recommended to take 'the experimental' course (Imagine a department of physics offering one course called 'the experimental' course!), with alternatives in child psychology, educational psychology, animal psychology, social psychology, abnormal psychology, advertising psychology, etc.—most of the latter conducted as reading or discussion or lecture courses with no introduction of the student first-hand to a child or a rat or a schoolroom or a social group or a moron. While employing experimental and other natural science methods in their research and in most of their discussions, psychologists lapse too readily when within academic walls into the traditional manner of presentation handed down from the days of psychology conceived as one of the branches of philosophy and paralleled with elementary logic and ethics. In consequence, it is unusual for the student to carry away with him the conviction that psychology does and can advance only as does any other natural science; and as for any very precise ideas as to the technique of psychological work he must be poorly supplied. In a word, can a student come to understand what psychology as a field of knowledge now-a-days really is unless he has had some first-hand acquaintance with experimental methods in the subject?

On the other hand, the student working in the typical 'experimental' course often feels that, beyond what natural interest he happens to have in technique and manipulation, there is little to be had here; he is aware of and often describes a disjointedness between his introductory and his later experimental study. The former is arm-chair work, often formal and verbal, and the latter is training in technique of investigation and verification of problems that are too often unarticulated within his psychological knowledge. One of the first needs, then, is to correlate—and not successively but simultaneously—psychological doctrine and theory with psychological methods and technique; building it all into one single course, with a definite number of lectures or recitations and a definite amount of laboratory time per week. The

organization of the two kinds of work as parallel and simultaneous implies that the subject-matter should be as closely correlated as possible, so that the experiment undertaken at the laboratory period of a given week may show a logical and fairly obvious connection with the theoretical readings and lectures of that same week. The student will then be in a position to take up the experiment assigned as the isolation for special treatment of a problem naturally arising in the course. When well chosen, moreover, the experiment will tend to clarify considerably classroom presentations. A student recently when asking the writer questions on a difficult topic in the lectures asked, "Can't we have an experiment on that point?" Now, this close synchronizing and correlating of classroom and laboratory need not call for excessive ingenuity; the writer has, in fact, found it relatively easy. Once the teacher has reconciled himself to pruning the great mass of experimental procedure on sensation that has been accumulated, and has reduced the students' laboratory work on this topic to correspond to the relative position given it in lectures and text, his task is then to find suitable work on the many other topics. Perhaps he will be helped by a further consideration. As this is the first and, for many students, the only course in psychology, it would seem well to bring in not only a range of general topics, but also incidentally a variety of methods found in use here and there in the working field of psychology. Somewhere in the series let children be brought in as subjects, at another time let animals; in connection with one laboratory period introduce simple statistical technique, with another the carefully interpreted drawing of microscopic views of important tisues; let the student work now with a partner, now in a large group, now alone; let him emphasize sometimes introspective observation, at other times objective.

To be sure, our undergraduate laboratories are inadequate; but the proposed plan will tend to cut down swollen enrollments in beginning psychology and this awful and dreaded result will be compensated for if administrative authorities can become definitely convinced that psychology is after all

a science with scientific needs for equipment and housing and instruction staff.

The difficulties in the way of establishing the experimental work with large introductory classes ought not to be greater than that involved in organizing laboratory work for other scientific departments. It should, indeed, not be so great in one respect: the zoologist must provide his students with the animals and the physicist must provide the mechanical forces which are the subject-matter of their respective studies, in addition to the technical apparatus for the study thereof, while the psychologist can expect his students to furnish most of their own subject-matter—their own human nature—and himself to furnish only the apparatus. Sometimes, too, the apparatus required is fairly simple, and frequently the same major pieces are usable for several different experiments, as a form of tachistoscope is adaptable to the study of perception, of attention, of memory, and of association. A cardinal principle implied in this whole plan is that the various members in a laboratory section should be driven abreast, should all be taking the same experiment at the same time or within the same laboratory period. This means, of course, much duplication of apparatus, but the gain in this method over the more usual plan of having only one or two pieces of each kind at hand and forcing the students to take up experiments in all sorts of orders is tremendous. For a student to be working on the same general problem as all his classmates heightens his morale, provides opportunity for group discussion and comparison, decreases teaching time enormously, and allows for the following by each student of a logical order of experiments, most expecially in connection with the point made above concerning parallelism between lecture and laboratory.

A word as to room accommodations. With the sections of introductory students a minimum would be a large well-lighted room furnished with a complete set of small working tables for two students each, if possible wired for electric current, a blackboard for use in preliminary and concluding discussions by the instructor, a demonstration table, a large

wall-clock with hand showing seconds which can be read by the students in place of the expensive and frequently-out-of-repair stop watches, and storage cabinets. For experiments in sound and sometimes in memory and in attention, isolation is called for, and a more elaborate plan necessary. One suggestion is that the large room used be a corner one, that the floor space near the two windowed walls be divided up by partitions to form small rooms for each student-pair and their table, closed by a door for experiments requiring isolation; but practically, these partitions can be dispensed with, where certain experiments (as auditory localization) are not included in the work.

This principle of the close interweaving of theory and abstract knowledge with experimental, controlled observational, and concrete work may be properly claimed to hold for all or nearly all the undergraduate courses in psychology. The subject-matter of different courses will of course dictate different combinations. Space will be taken only to suggest a few of the very many possibilities in the different subjects, and references will be made to certain personal cases in order to exhibit the practicality of the suggestions.

In *educational* psychology courses a wide variety of experimental and observational material is of course available. The writer has found student interest heightened strikingly by the use of a continuous learning experiment occupying the first five minutes of each class hour, each student keeping and graphing his own results, the instructor showing class or sub-group results on a large wall chart. Even more definite a departure from traditional methods—and more clearly a success—has been to make a chosen series of class and individual experiments *central*, with discussions and lecture material centering largely about the laboratory reports. Besides well-known experiments in motor habit forming, memory, attention, etc., intelligence tests and statistical methods of studying individual differences are some of the forms of concrete work available in educational psychology; and in addition to the class members, children should ordinarily be obtainable for subjects.

In *child* psychology the most successful method in the writer's experience was to place in the student's hands a printed set of instructions for measurement of physical, reflex, instinctive, emotional, sensory, and the various intellectual traits; with the plan for the student to obtain, say, two children of different ages for week to week study; following out the printed instructions as per schedule and in parallel relation to the classroom lectures. Another form of concrete work was to allow different individuals special problems to pursue, getting as many children as possible for their subjects. Then, of course, there remain the methods of classroom demonstration with selected children, of playground and school room observation of children; and of familiarizing the students with the methods and apparatus of child analysis by letting them try them out on each other—this where finding child subjects is difficult.

In *animal* psychology a fixed hour or hours per week for work with animals kept for the purpose—white rats, guinea pigs, cats, etc.—helps to give concrete basis to the course. The writer has found definitely useful class experiments in problem box or maze learning, with the students forming working shifts, and each one keeping all the results and formally writing up the experiment as a course paper; also individual observations of systematic and guided nature on reflex, instinctive, and emotional tendencies with animals easily obtainable for the laboratory.

For a course in the psychology of *advertising* and *selling*, many of the well-known laboratory experiments and demonstrations in attitudes, preparatory and consummatory responses, suggestibility, attention-attracting and attention-holding, etc. are readily available. A colleague of the writer uses with excellent success the individual student's collection and analysis of advertisements appearing in past and current magazines and newspapers, according to principles brought out in his lectures. Then there are the 'historical method' recently brought to public attention; the study of billboards about town; perhaps experimental advertising through student paper and campus bulletin boards. The

study of salesmanship, touching as it does on the subject of personnel, has many concrete possibilities.

A course in *personnel* can of course easily include training in the use of various types of psychometric tests (Whipple, Link, Downey, etc.). Another colleague of the writer finds student coöperation in the refining of his personality rating scale of mutual advantage, the individual's raters being selected not only from his classmates but also from his friends and relatives. It is anticipated that with the revival of prosperity the installing of personnel offices in industrial establishments will be useful indirectly as offering opportunities for observations on the ground; while university organization of vocational guidance study with the coöperation of the psychologists offers another field.

For courses in *abnormal* psychology, it is obvious that the human material is not so readily available for class use, but the visiting of hospitals after careful class preparation on points for observation has been tried satisfactorily by many. A colleague has found quite convenient also the student's careful study of and familiarization with the instruments and technique of mental examination, and the careful analysis and diagnosis of true or hypothetical cases of which the raw data is furnished the student. The student's plotting of family trees on the basis of facts obtainable by correspondence with his relatives and friends can be done well with blanks secured from the Eugenics Station at Cold Spring Harbor.

In his work in *social* psychology a colleague has obtained valuable material as well as given the students concrete contact with their subject by the method of experiments on group *versus* individual work, and on individual reactions to facial expressions. Concrete work can be done also along lines of statistical study of greatness in leadership, etc. (Cattell and Galton); of racial differences discoverable by Binet, Downey, and other tests; of suggestibility factors by class experiments; etc.

Concrete work in *legal* psychology can be done with *aussage* experiments on members of the class under varying

conditions and with children of different ages; with experiments on trade-mark and other infringement; and with experiments on detection of guilty knowledge by free association, blood pressure, inspiration-expiration ratio, psychogalvanic phenomena, etc.

Finally, the writer wishes to propose the establishing of a type of course supplementing the foregoing and serving as the legatee of the traditional 'experimental course.' His experience is that undergraduates with a certain minimum of training in experimental technique and with definite interests of their own can in many cases carry through satisfactorily carefully limited minor pieces of psychological research, under a professor's constant oversight. Not infrequently, such carefully supervised work has established something sufficiently definite in this or that little corner of a problem for it to be incorporated into a larger investigation in progress in the laboratory. And from the student's standpoint it proves valuable in two ways. As to its effect upon interest and enthusiasm in psychology the evidence is almost uniformly favorable. As to its importance in acquainting the student with psychology-in-the-making there can be no pedagogical question. Such an '*experimental problem*' course might best be open to students after finishing both the introductory and at least one advanced course; also possibly to gifted students after the introductory double-course alone.

Such suggested changes to introduce more of the concrete observational and experimental material mean extra teaching time—but not necessarily of professorial grade—and especially, adequate laboratory facilities. The degree to which psychology has lagged behind the other sciences in its obtaining of room space and equipment is too well known to call for elaboration.

This paper has urged the introduction of concrete and experimental work in psychology because it is properly a natural science, but the former recommendations do not stand or fall on the latter interpretation: even he who is not willing as yet to allow psychology complete scientific status in the curriculum must recognize the importance of the pedagogical

principle that emphasizes the direct study of the subject-matter itself as well as or even in preference to the study of theories about this subject-matter. Instead of merely reading *about* things we should get into direct contact with things themselves; and the high and dry study of what other men *say* about the phenomena in which we are interested should be at least supplemented by our own personal observation of the phenomena. The proper study of psychology is first and last the human phenomena themselves.

## Sample Organization of the Experimental Side of the Introductory Course

With the purpose of merely submitting a concrete example of how some of the ideas presented in the foregoing pages have been given actual form and found to work satisfactorily, the writer offers an outline sketch of this introductory course as organized at the University of North Carolina. The elementary work in psychology is in the form of a two-quarter course, totalling about ninety-five to one hundred class-hours; it thus parallels the elementary offerings in the other natural science departments on the campus. It is in essence a substantial expansion of the well-known one-quarter lecture-text-book course plus the absorption of a main part of the usual experimental course. And it is all organized into a single systematic covering of the ground with experiments paralleling lectures instead of being taken in a subsequent quarter. Lectures are given to the class as a whole four, sometimes five, times per week; and the experimental work is offered for one two-hour period per week in the laboratory, the class being divided into sections of twenty or less. With a sufficient duplication of apparatus, all the students perform the same experiments at a given period. (This is in some cases rendered somewhat simpler by setting two minor experiments for the period, duplicating apparatus in each case only sufficiently for ten students, and having the two divisions of the sections exchange problems and apparatus at about the middle of the period.) Of course, an increase of teaching time is entailed; this is provided for by an instructor-

ship assigned to the one duty of conducting these sections, the lecturer usually being present with or even conducting the first section of the week. Written formal reports of the experimental work done, with presentation of all data and guided discussion of data and of general problems, are made by the student, and are due on a specific day of the week following. It may be remarked that as the topics and experiments have here been arranged, the students do not need any peculiar and special initiation into the mysteries of psychological experimentation, but carry over sufficiently their high school and college training in other experimental sciences.

| *Lecture topics* (those paralleled with experiments) | *Experiments* |
|---|---|
| Psychology as Study of Human Stimulus-Response Phenomena | Reaction times,—simple, choice, perceptual, and association<br>Materials: 5 pendulum chronoscopes, 5 Woodworth-Wells color naming tests, 5 sets Kent-Rosanoff stimulus words |
| Effectors | Muscular fatigue, and Muscular coördination<br>Materials: 5 finger dynamometers (Titchener, home made); 5 kymographs (Harvard); 1 bell-metronome; 5 sets target blanks (Whipple) |
| Cerebrospinal Division of Nervous System | Drawing of brain parts<br>Materials: 10 plaster models of dissectible brain-in-cranium; 1 preserved natural specimen; 1 wall plaque |
| Reflex Actions | Identification of reflexes (wink, knee-jerk, pupil), and some phenomena<br>Materials: 5 winking glasses (Partridge, set up with rods and clamps); 5 knee-jerk hammers (home made and set up at |

| | |
|---|---|
| | table edges with rods and clamps); 1 hand dynamometer (Smedley); 5 small black cards; 5 millimeter rules |
| Complications of Reflex Action | Delayed reactions<br>Reflex pattern-reaction to smell<br>Materials: 5 sets target blanks (Whipple); 5 sets odorous substances—very unpleasant and very pleasant |
| Instinctive Action | Instincts in animals<br>Materials: 10 or more white rats both hungry and fed; 1 maze allowing much exploratory behavior; 1 choice chamber with open and enclosed passages; 10 small wire screens; 1 small swimming tank of lukewarm water; etc. |
| Emotions | Psychogalvanic phenomenon<br>Materials: 1 galvanometer (taken as a group demonstration) |
| Emotions and Feelings | Facial 'expression'<br>Musical 'expression'<br>Materials: 10 sets of photographs (Feleky), with tables of standard judgments; 1 phonograph and selected set of records |
| Feelings | Esthetic judgments<br>Materials: 10 sets geometrical designs (Fechner and additions) |
| Attitudes and Attention | *Einstellung* phenomenon<br>Laws of attracting attention<br>Materials: 10 sets printed arithmetic blanks (specially devised); 1 bell metronome; 10 simple card exposure apparatus (home made); 10 sets printed cards (Starch) |

| | |
|---|---|
| Intelligence: Statistical Methods | Intelligence tests<br>Materials: standard group tests |
| Learning | Habit forming: star tracing<br>Materials: 20 mirrors, rods and clamps, 5 stamps for stars (Starch) |
| Learning | Habit forming: maze running<br>Materials: 10 fibre board mazes with glass styluses |
| Learning | Memory<br>Materials: 10 tachistoscopes (Dearborn-Langfeld); 10 sets printed lists syllables and words |
| Learning | Implicit habits: free and controlled associations<br>Materials: Printed stimulus-words, tables of types of association, etc. |
| Sensations | Visual: color mixing; retinal zones<br>Materials: 5 hand color wheels (Milton-Bradley); 5 perimeters (Stoelting No. 12423) |
| Sensations | Auditory: analysis of musical capacity<br>Materials: 1 phonograph; 1 set records (Seashore) |
| Sensations | Olfactory: Exhaustion<br>Kinesthetic: Weber's Law<br>Materials: 5 sets iodine, camphor, etc.; 5 sets variable weights (shells and shot); 1 analytical balance |
| Imagery | Imagery: types and attributes<br>Materials: mimeographed questions (Galton, Seashore) |
| Perceiving | Perception span<br>Cutaneous space<br>Materials: 10 tachistoscopes (Dearborn-Langfeld); 5 esthesiometers (compass) |

| | |
|---|---|
| Conceptual Reactions | Development of concepts<br>Materials: several children of different ages; lists of words (class demonstration) |
| Reasoning | Problem solving<br>Materials: 5 puzzle boxes (Healy); 10 labyrinths (Tait) |
| Volition | Inhibition<br>Types of decision<br>Materials: Hard directions tests; printed 'dilemmas' (Martin) |

All the above-mentioned materials have been furnished on an outlay of less than $1500. While this may seem to some a rather large investment for the introductory course in psychology, attention is directed to the expense incurred in organizing even a high school chemical, physical, or biological laboratory. To be sure, several of the pieces of apparatus listed above can be duplicated after a fashion with quite simple materials, but this has two disadvantages. Quantitative results—and these should be emphasized even in introductory work—are much more uncertain; moreover, such are the experiments performed with merely cardboard, pencil, and a piece of string are likely to be taken in a more perfunctory spirit. It should be borne in mind further that some of the duplicated pieces of apparatus listed are of excellent use in connection with the experimental work in other courses than the general introductory, and can thus be given double use with a little attention to the details of course programs to prevent conflicts in needs.

As implied in the earlier part of the paper a great need of psychology teachers today is that they become more scientific-minded *academically* and thus teach the undergraduate to be scientific-minded in his conception of psychology.

mjc